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EDITED BY HENRY LAWSON, M.D.

M.R.C.P. Edin., F.R.M.S., Lecturer on Physiology, and Assistant-Physician to St. Mary's Hospital.

CONTRIBUTORS.

Adams, A. Leith, M.A., M.D., F.L.S.
Ansted, Professor, F.R.S., F.G.S.
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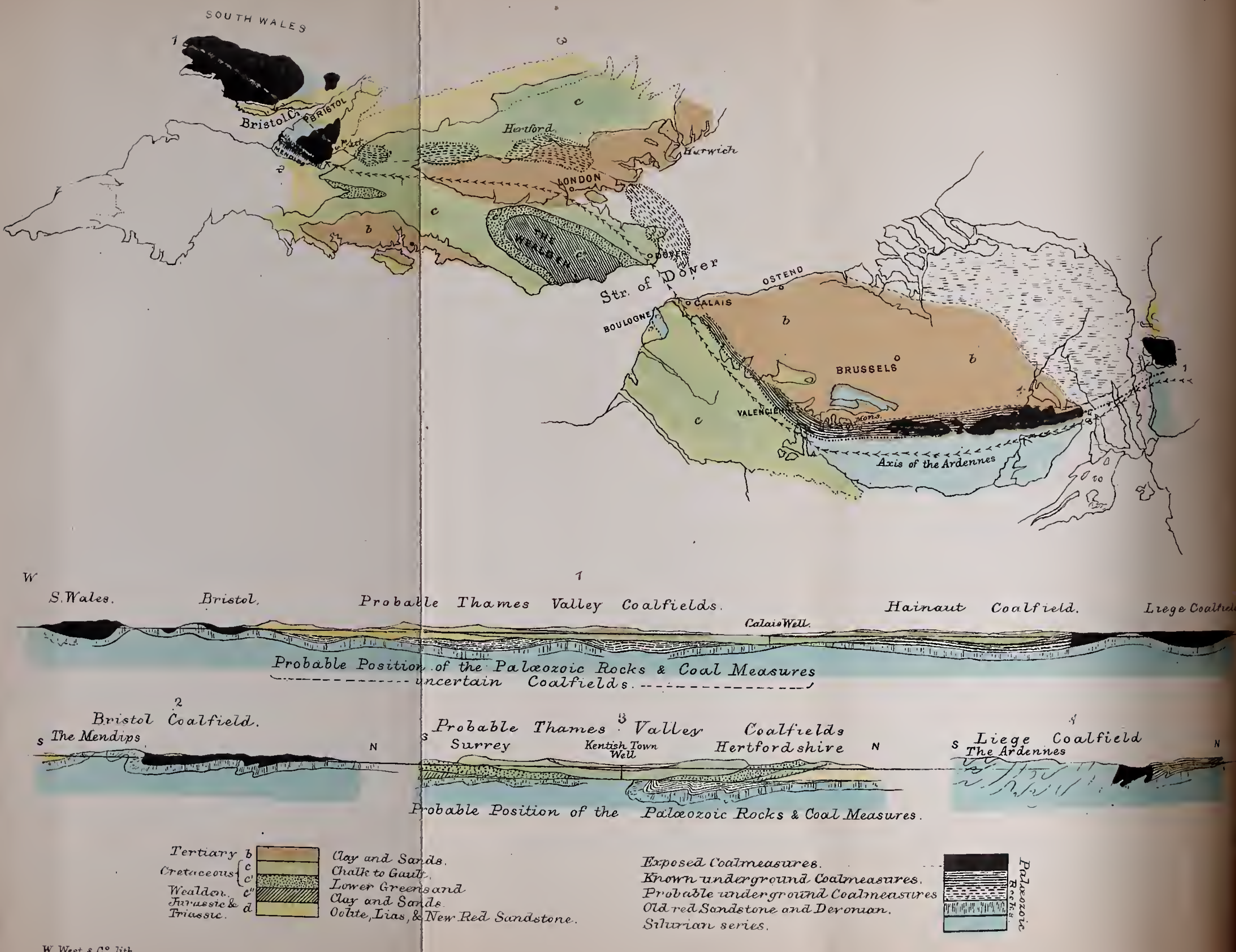
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(13.)

ON THE PROBABLE EXISTENCE OF COAL MEASURES IN THE SOUTH-EAST OF ENGLAND.

By JOSEPH PRESTWICH, F.R.S., F.G.S.

[PLATE LXXXV.]

THE question of discovering coal in the south-east of England, so long treated in an empirical manner, has of late years assumed a scientific aspect, but in a direction entirely different from that towards which practice pointed. So many of our Tertiary and Secondary strata contain thin beds of lignite and shale, together with beds of sandstone—the first sometimes not unlike impure coal, and the latter resembling the shales and sandstones of the Coal Measures—that it is not surprising that men, ignorant of scientific modes of investigation, and at a period when geology was little understood, should have been misled by resemblance of parts into a belief of identity of the whole. But the progress of geology has since conclusively shown that although certain beds of lignite of Miocene and Wealden age may be worked for such purposes as lime-burning, and some beds of fair coal are wrought in the Oolites, as at Brora in Sutherlandshire, yet to all practical intent the beds of good and workable coal are confined to certain strata, known as the Coal Measures, forming the upper part of the *palæozoic* series of rocks, the position of which is perfectly well defined, and the organic remains of which serve to render their determination a matter of certainty.

The imperfect lignites of our tertiary strata around London; the lignites and sandstones of our Wealden area; the clays of the Jurassic series, have all at times given rise to the search after coal in the strata far above the Coal Measures; while the sandstones of the Devonian and the shales of the Silurian series have given rise to equally abortive attempts amongst the rocks under the true coal measures, even up to a recent period, by so-called practical men.

Aubry gives a curious account of one of these searches after coal, made in the neighbourhood of Guildford, at the end of

the seventeenth century. "The Rev. Giles Thornborough, one of his Majesty's chaplains, digging a boring for coal in Slyfield Green, found, first of sand and gravel 7 ft.; then a spring; within a little of that a bed of stones like square caps and about 2 ft. every way, on the outside whitish, within full of sulphur, out of which was extracted tinn by L. Smyth, of London, engraver. These stones are called at the coal pits at Newcastle, 'catts' heads,' lying always (they say) where coal is. These catts' heads are all full of small pipes for the mine to breathe through. Next under them lay a body of black clay for 15 fathoms; then a rock of stone about a yard thick, which was very hard. Then they came to black clay again for about 3 fathoms, and then another rock; after that, clay mixed with minerals (of which Prince Rupert hath some, as also had King Charles in his closet, which was there placed by the Indian oar); then cockle shells, muscle shells, and periwincle shells, some filled with oar (out of which Prince Rupert extracted tinn and other things), and some filled with clay. After this sprung a bed of oker, 12 ft. thick, a kind of mother-of-pearl; after that, a green quicksand. Then came coal, which how deep it is, is unknown, for here the irons broke: thought by Mr. W. Lilly (astrologer) to be subterraneous spirits; for, as fast as the irons were put in, they would snap off. This is a kind of rocky coal (like that which they call Kennell coal) which burns like a candle." Mr. Thornborough was induced to make this trial, because "there was a kind of stony coal (lignite) that would burn, which he found by grubbing up the roots of an old oak in his ground there." After spending 400*l.* the work was abandoned, to be however revived again about a century and a half later, in the neighbourhood of Worplesdon.

The Wealden strata at Bexhill in Sussex consist of a series of sandstones, clays, and lignites, which led in 1804 to an expensive trial for coal near that village. A shaft was sunk to the depth of 164 ft., and two seams of lignite, designated as smut coal, 2¼ ft., and strong coal, 3½ ft. thick, were met with. The reputed resemblance to coal measures was kept up by the use of such terms as "church clay," "grey bind," "blue bind with iron ore," &c., and the presence of a seam of clay with impressions of wealden ferns not unnaturally assisted the delusion. Fortunately probably for the company, the mine was drowned out, and stopped the further extension of the work.

So recently as about thirty years since, another attempt was made to get up a company to establish a colliery at Worplesdon, near Woking, in the Bagshot Sands, and to sink a shaft to the depth of 150 ft. At that depth the projectors would have reached the London clay; but it is certain that no more profitable material would have rewarded their outlay. Any geologist could at

that time have told them what their chances were ; nevertheless, they state, " It does not appear that there has been any actual geological investigation or survey of the county of Surrey; therefore it is proper to observe that the non-assignment of coal to that county in the maps which profess to give the geological character of England is not a matter of importance. The most scientific geologists admit that there are various unexplored localities which future research may add to the coal-fields already known." The projectors seem also to have found encouragement in the prospect of being able "to work a mine so near Windsor."

While practice has been making during the last two centuries these tentative efforts—efforts even now continued from time to time almost as blindly as in former days*—science in the meantime has been making slow but sure advances, and is now prepared with an hypothesis respecting the existence of coal in our southern counties of very great probability. William Smith first established the order of superposition—confirmed by succeeding geologists—of the secondary and tertiary rocks of the south of England, the thickness of which at their point of outcrop in or nearest adjacent to the London basin may be roughly estimated as under :—

		Order of Succession of the Strata.	Locality of estimated Thickness.	Average Thickness.
Tertiary Series.		Bagshot Sands . .	Surrey	300ft.
		London Clay . .	Middlesex . . .	400
		Sands and Mottled Clays	Surrey	80
Secondary Series	Cretaceous.	Chalk	Hertfordshire . .	1000
		Upper Greensand . .	Surrey	30
		Gault	Surrey	120
		Lower Greensand . .	Surrey	500
		Weald Clay and Hastings Sands	Sussex	2000
	Jurassic.	Purbeck and Portland Beds	Berkshire, Oxfordshire	70
		Kimmeridge Clay . .	Buckinghamshire .	450
		Coral Rag, Oxford Clay .	Wiltshire to Oxfordshire	500
		Oolites	Gloucestershire . .	750
		Lias	Gloucestershire . .	600
	Trias.	New Red Sandstone .	Somersetshire . .	800
				7,600

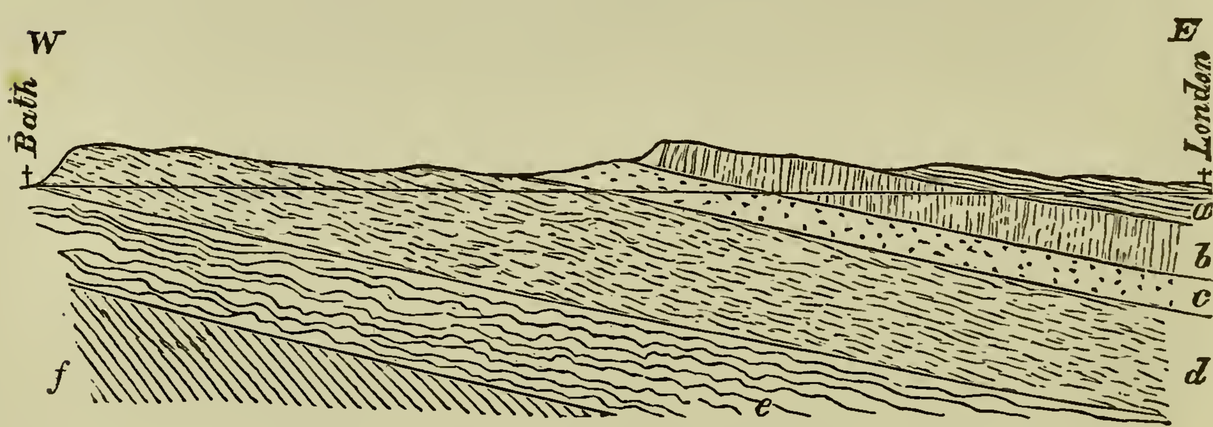
* Even up to the present day a firm belief exists among many, even of men experienced in coal-workings, that in the lower tertiary strata between the London Clay and the Chalk, good coal exists. As hundreds of wells and sections innumerable expose these strata, we well know how futile any such expectations, founded on the presence of irregular seams of lignite, are.

And since then the order of the older underlying rocks has been thus determined by Sir R. Murchison, Professor Sedgwick, and others :—

Palæozoic Series.	Permian		Gloucester, Warwickshire
	Carboniferous	COAL MEASURES	Somerset, Gloucester
		Millstone Grit	Somerset, Gloucester
		Mountain Limestone	Somerset, Gloucester
	Devonian (Old Red Sandstone)		Devon, Somerset
	Silurian		Wales
	Cambrian		Wales

It was for a time supposed that secondary strata maintained in the main their regular sequence and thickness unimpaired over large areas, and in our early geological works the section of the secondary formations, from the west to the east of England, is given as under :—

FIG. 1.



a. Tertiary strata. *b.* Chalk. | *c.* Lower Greensand. *d.* Jurassic series. | *e.* Lias and Trias. *f.* Palæozoic rocks.

In this case, the depth to the *coal measures* under London, supposing they existed there, would have been from 7,000 to 8,000 ft. or more. Speculation was hushed in presence of such depths, nor did it actively revive until the facts more recently acquired showed that the obstacles presented by such an enormous mass of strata had no real existence.

Admitting the variation of thickness, we were hardly, however, until lately, prepared to admit how rapid that variation was. Professor Hull has shown that the Lias and Oolites become much thinner as they range eastward from Gloucestershire. There is reason to believe, in fact, that the oolitic series do not extend far under the chalk hills of Berkshire, and it is known that the Inferior Oolite thins out even before reaching Oxford. Mr. Hull gives a section, from Gloucestershire (the neighbourhood of Cheltenham) to Oxford, in which he shows that all the rocks below the Great Oolite thin out rapidly to the south-east ;

and he estimates the thickness of the whole at Oxford at about 600 ft., whereas in Gloucestershire it is 1,880 ft.:—

	Gloucestershire. Feet.	Oxford. Feet.
Great Oolite and Fullers Earth .	370	205
Inferior Oolite and Sands .	420	0
Lias (Upper and Marlstone)	640	200 ?
Red Sandstone and Marls (Keuper)	450	200 ?
	1,880	605

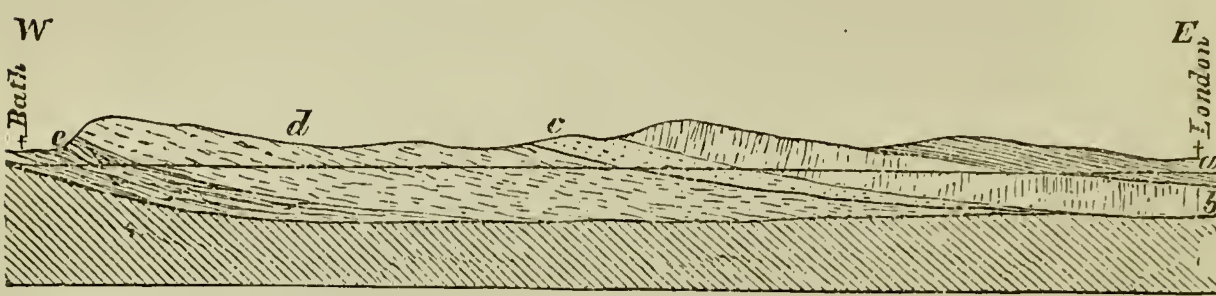
To the north-eastward the Great Oolite and the Oxford Clay become but slightly thinner. On the other hand, the Kimmeridge clay, which is 275 ft. thick at Swindon, is 310 ft. near Abingdon, and increases to 450 ft. at Aylesbury. In like manner, the Portland and Purbeck series increase from 12 to 70 ft.; while the Wealden series, which is altogether absent in the west of England, is about 2,000 ft. thick in Surrey and Sussex. So the Lower Greensand, which is only 50 ft. near Devizes, attains a thickness of about 500 ft. at Reigate. The Gault maintains a mean thickness of about 100 ft.; while the Upper Greensand, 150 ft. thick at Devizes, is reduced to 25 ft. at Merstham. The Chalk, taken at its full development, maintains tolerably constant dimensions from Wiltshire to Dover, viz. of from 800 to 1,000 ft. unless when, as often happens, it has suffered denudation. Notwithstanding, therefore, the large development of the Secondary formations, both westward and northward of the London basin, it was uncertain how many and how much of these might be found to extend under the Tertiary strata of the south-east of England. We did not anticipate, however, the great hiatus which in reality has been found to exist. Whatever might be the case with the triassic and jurassic series, it seemed at all events probable that the lower cretaceous strata, which are so fully developed a few miles both on the north and south of London, would, like the upper cretaceous strata (the chalk), pass under London, and therefore it was a matter of surprise when the boring at Kentish Town, made in 1854, showed that not only were the older secondary strata all absent, but also the lower greensands, which are so well developed in adjoining parts of Kent and Buckinghamshire. In the place of the latter were found a series of red and grey sandstones belonging probably to the Old Red Sandstone, so that the following diagram (fig. 2) may now be considered as representing probably the order of succession and position of the strata from the West of England to London:—

In the same way MM. Dufresnoy and Elie de Beaumont*

* "Explication de la Carte Géologique de la France, 1841," vol. i. p. 727.

have given a diagram showing a similar thinning out and overlapping of the strata from south to north in the north of France, and a like unconformable independence of the secondary

Fig. 2.



a. Tertiary strata. | *c.* Lower greensand. | *e.* Lias and trias.
b. Chalk. | *d.* Oolitic strata. | *f.* Palæozoic strata.

and of the palæozoic series, only there the older strata come to the surface, which they do not in south-eastern England :—

Fig. 3.

Westward of this line of diagram in France, the palæozoic rocks sink deeper, and are altogether covered by secondary and tertiary strata ; and while at Lille the *chalk* is found to repose on the *mountain limestone*, at Valenciennes it reposes on *coal measures*. These latter are the prolongation of the great Belgian coal-field, which ranges from Liège westward to and beyond Mons.

It is about two centuries ago that the Belgian coal-basin was found to pass beneath the newer formations westwards in the direction of France, where the Coal Measures were supposed to be lost under these Cretaceous and Tertiary strata. It occurred, however, to some more far-seeing men that they might possibly be recovered, and, after various trials, their search was attended with success, the strike of the coal measures hit upon, and valuable collieries established at Anzin and Aniche, in the neighbourhood of Valenciennes. This encouraged further search, and the coal measures have been gradually followed in a westerly direction under the chalk to within thirty miles of Calais, and at that town a boring for water, made by M. Mulot in 1842, proved the presence of carboniferous strata (but not the coal measures) under the

tertiary strata and the chalk, to a depth of 1,138 ft.* Again, under the same strata at Ostend, palæozoic rocks have been discovered at a depth of 985 ft.† On the English coast, a boring for water was carried at Harwich to a depth of 1,070 ft., also through tertiary strata and chalk, which were found to repose upon a fossiliferous slate belonging to one of the lower members of the carboniferous series. We can thus follow at intervals the same Tertiary and Secondary strata overlying *unconformably* Palæozoic strata from Mons to London. The order of superposition and ascertained thickness of the several overlying formations and the nature of the fundamental rock at these points, is shown in the following table :—

	Kentish Town, London	Harwich	Ostend	Calais	Hames, near Calais	Anzin, near Valenciennes	Flines Nord	Ville Ponveraeul, near Mons
Tertiary Strata . .	324	76	682	241	6		140	54
Chalk	645	888	210	762	629	265	390	844
Gault and Greensands .	144	61	93	29	33		3	138
	1,113	1,025	985	1,032	668		533	1,036
	OR	ML	OR	ML	OR	C	ML	C

C = *Coal Measures*, ML = *Mountain Limestone*,
OR = *Old Red Sandstone or Devonian.*

There is little doubt from all we now know that, owing to the absence of all the lower secondary formations the great Tertiary and Chalk plains of Belgium and French Flanders repose directly upon a floor of old Palæozoic rocks, and that a like structure obtains in the London basin, at all events as far as London. The reasons we have for believing that the coal measures are associated with this palæozoic base, we will give further on.

In England, south of a line drawn from Bath to Stamford and Yarmouth, no true coal has yet been found. The whole area is occupied by strata newer than the Coal Measures, commencing with the Liassic and Oolitic series to the east of Bath, and ending with the Chalk and Tertiary series of the neighbourhood of London. Nevertheless, in consequence of certain presumed relations between the coal-fields of Belgium and of

* "The Water-bearing Strata around London," p. 208.

† "Bull. Soc. Paléont. de Belgique," Antwerp, 1859. At Vilvorde, near Brussels, Silurian or Devonian rocks were also met with at a depth of about 600 feet.

the north of France with those of South Wales and Somersetshire, an opinion has for some time prevailed amongst geologists that coal probably exists in parts beneath the newer formations of the south of England.

So long since as 1826, Dr. Buckland and Mr. Conybeare, in their excellent account of the Bristol coal-field,* made the following remarks: "Before we close this general account of the south-western coal district of England, we are desirous of noticing its resemblance in geological structure and picturesque features to the country extending along the Meuse between Namur and Liège. There also we are presented with coal basins encircled by Mountain Limestone, and based on Old Red Sandstone, which latter is displayed at Huy. These rocks are all highly inclined, and are covered by overlying formations. The defiles of the Sambre and Meuse present exact counterparts to those of the Avon and Wye." The relation of the two areas was more particularly noticed in the work just quoted (pp. 724-5) by the eminent geologists MM. Dufresnoy and Elie de Beaumont, who thus express themselves on the theoretical question of the original extent of the coal-fields of Belgium and the north of France, and on their probable range:—"The portions of carboniferous strata, which we have reserved as the subject of the last parts of this chapter, contrast in an important manner with those which we have hitherto described, inasmuch as they do not exhibit the characters of deposits formed in circumscribed basins; all indicating, on the contrary, that they have been deposited in an open sea. From the Ardennes on to the mountains of Wales and Scotland, there extended at that period the bays of a sea, in which the Mountain Limestone was formed, which contains a large number of marine remains, and after that the coal measures of the north of Belgium and a part of England."

"This difference in the character of the two classes of basins of the coal measures of which we have just spoken, is not only an interesting fact for science, but it concerns also in a great degree the future of mineral industry, from the views it may suggest relative to the possible subterranean connection between certain basins. In fact, the deposits formed in circumscribed basins present but little chance of their being prolonged for any considerable distance beneath more modern deposits, but the deposits formed in marine basins are generally much more uniform, and susceptible of much greater extension, when they have not been broken up and destroyed."

* "Geological Transactions," vol. i. pl. 2, 2nd ser. p. 220.

Again, M. Mengy, in 1852,* remarks: "However it may be, the considerable thickness of the overlying unproductive strata (*terrains morts*) which exist in the western part of Belgium and Department du Nord, shows that there is a great depression there, which is a prolongation of the deep subterranean substrata above which London stands, and if a coal measure basin exists there, which is not impossible, they may extend to the southern border of that depression, which reaches near to Lille, and may be connected more or less directly with the vast carboniferous system which comes to the surface in England, ranging from Wales to Scotland."

But it was not until 1855, when Mr. Godwin-Austen † brought the question before the Geological Society in an able and elaborate paper, accompanied by a map, in which he showed that the coal measures which thin out under the chalk near Théronanne probably set in again at or near Calais, and are prolonged (beneath the tertiary strata and the chalk) in the line of the Thames Valley, parallel with the North Downs, and continue thence under the valley of the Kennet, into the Bath and Bristol coal area, that the attention of geologists was seriously directed to the subject. Reasoning also on theoretical considerations connected with the extension of the old coal-growth in the west of Europe, Mr. Godwin-Austen concluded that "coal measures might possibly extend beneath the south-eastern part of England," and he showed, upon well-considered theoretical grounds, that the coal measures of a large portion of England, France, and Belgium were once probably continuous, and that the present coal-fields were merely fragments of a great original deposit, which he inferred had been broken up in two directions previously to the deposition of the secondary rocks. He endorsed also the opinion that the main line of disturbance had a general east and west direction—that part of it formed the great anticlinal of the Ardennes, by which the Belgian coal-field had been tilted up, and brought to the surface—and that the Mendips with the Somerset coal-field are on the same line of strike.

These views have been controverted by some distinguished geologists, but they have received the assent of a greater number; and the information we have since acquired of the thickness of the secondary strata and of the existence of palæozoic rocks at Kentish Town and Harwich,‡ and the discussion

* "Essai de Géologie Pratique sur la Flandre Français," 1852, p. 76.

† "Quart. Journ. Geol. Soc.," vol. xii. p. 83.

‡ For detailed particulars of the strata at these wells, see the papers by the writer in "Journ. Geol. Soc.," vol. xii. p. 6, and vol. xiv. p. 249.

of the subject by the Royal Coal Commission,* enables us now to investigate the physical problems with still greater certainty.

It is evident from what we have already said that the age and position of the surface rocks afford no criterion of the thickness of the strata intervening between that surface and the palæozoic rocks underlying the Tertiary and Secondary strata of the south of England; while it is also clear that the relation of the secondary and of the palæozoic group of rocks to one another is perfectly independent, and that the latter must be viewed entirely on their own internal evidence, apart from the bearing of the newer rocks which cover them unconformably.

A glance at the geological map of Europe will show that the Belgian coal-field is but part of a series of great coal-fields ranging from Westphalia to the north of France. These coal-fields are deep, long, and narrow, and their longer axes succeed one to another on the same line of strike.* Omitting a few small unimportant coal-basins, the most easterly of the great coal-fields is known as that of the Ruhr, the second as that of Aix-la-Chapelle, the third as that of Liège, and the fourth as that of Hainaut and Valenciennes. In all these districts the Coal Measures are tilted up or faulted on the south against the Mountain Limestone or the Devonian rocks, or pass northward under Cretaceous and Tertiary strata, beneath which they are prolonged until thrown out by other undulations of the older rocks. The width, north and south, of these coal-fields is always small compared to their length. Thus the coal-field of Liège is only 3 to 8 miles wide, whereas it has a length of 45 miles. So the exposed coal-field of Hainaut, from Namur to beyond Charleroi, is 33 miles long; it then passes under the Cretaceous and Tertiary strata, and is prolonged, with a few small exposures, underground, by Mons to Valenciennes. The length of this other underground portion of the coal-field is 32 miles, making a total of 65 miles, with a width near Namur

* Vol. of Evidence taken before Committee D, and Report to that Committee "on the Probabilities of finding Coal in the South of England" by the writer; Royal Commission on Coal Supply, 1871. Free use is made in this paper of this report and also of the writer's anniversary address to the Geological Society for 1872.

† These are given generally in the map, Pl. LXXXV., the main features of which are copied from Dumont's Geological Map of Europe. To them are added the ascertained range of the Coal Measures under the Chalk of the North of France and their probable range under the South of England. To this I have added a series of hypothetical sections along the line of the coal trough and across it, showing the possible disposition of the Coal Measures and the thickness of the overlying strata.

of 2 miles, increasing to 7 or 8 miles near Charleroi, and continued in France with a width of from 6 to 7 miles, where it has been followed under the chalk to within 30 miles of Calais, and there thins out.

Connected with these coal-fields a great line of disturbance, affecting the palæozoic rocks, has been traced from Westphalia through Belgium to northern France, and it is on the northern flanks of the older rocks of the Ardennes range of hills, which have been formed by this disturbance, that the coal-fields of Belgium lie. The same line of disturbance is again exhibited in the Mendips, and is prolonged even as far as the south of Ireland.

In England and South Wales a similar set of phenomena are met with at this other end of the axis of elevation. From Milford Haven to Tenby, contorted strata of the Mountain Limestone and Old Red Sandstone are flanked on the north by the highly-disturbed Pembrokeshire coal-field, which is 24 miles long by 3 to 6 miles broad. The great coal-field of South Wales is 60 miles long by 15 to 18 miles broad; whilst that of Somerset and Gloucestershire (or Bristol and Bath) shows a length in the direction of the axis of the Mendips of about 12 miles, and in the other direction it measures 26 miles.

The Coal Measures of South Wales are not covered by secondary strata, but a large portion of the Somersetshire coal measures are overlaid by some of the lower Secondary rocks, which in their turn pass a few miles to the eastward under the Chalk. At Clandown, near Bath, the Coal Measures are worked beneath 360 feet of Lias and New Red Sandstone, and they have been followed under these superincumbent strata for a distance of 5 to 6 miles from their outcrop, where they are at a depth of about 500 ft. beneath the surface. But between this point and the well at Kentish Town, no trial for coal or water has been carried to the base of the secondary rocks, or has reached more than about 600 ft. beneath the sea level, and the whole area extending to the channel is occupied by upper Secondary or by Tertiary strata.

There can, however, be little doubt of the continuity of the range of the palæozoic rocks under these newer formations from Belgium to Somerset; but whether or not the Coal Measures were ever continuous between the two districts; and whether, if they were, they have been removed by denudation, leaving only the lower palæozoic rocks, requires further discussion.

So far as the identity of any particular bed of coal or of rock may serve to establish a correlation between the coal measures of Bristol and South Wales and those of France and Belgium, it is not possible, nor should we expect it; for the variation in

all the beds of any coal-basin is well known to be so great and rapid, that, in the different parts of the same basin, it is often difficult, and sometimes impossible, to establish any correlation, while in adjacent basins, such as those of Wales and Bristol, or of Hainaut and Liège, such attempts have, with few exceptions, hitherto utterly failed. There are, however, general features which serve to show some relationship. The great central mass of from 2,000 to 3000 ft. of rock called Pennant exists in both the Welsh and Bristol coal-field; and the total thickness of Coal Measures is not very different, being, say, 10,500 ft. in the one, and 8,500 ft. in the other, with workable seams of coal, 76 in Wales, and 55 in Somerset. In the Hainaut (or Mons and Charleroi) basin, the measures are 9,400 ft. thick, with 110 seams of coal; in the Liège basin 7,600 ft., with 85 seams; and in Westphalia, 7,200 ft., with 117 seams. On the other hand, none of our central or northern coal basins, with the exception of the Lancashire field, exceed half these dimensions, and more generally are nearer one-fourth. Further, the difference which exists between the north country coals and those of Wales and Somerset, the preponderance of caking-coals in the former, and of anthracite, steam, and smiths' coal in the latter, equally exists between our north country coals and those of Belgium, which latter show, on the other hand, close affinities with those of Wales and Bristol. I am informed by two experienced Belgian coal-mining engineers and good geologists, who have twice visited our coal districts, that the only coals they found like those of Belgium were the coals of South Wales and Radstock; there was the same form of cleavage, the same character of measures, and the same fitness for like economical purposes. Organic remains afford us little help; and not sufficient is yet known of their relative distribution. The plants, are, as usual, the same; so also are shells of the genus *Anthracosia*, and a number of small *Entomostraca*; and the marine forms are scarcer than in some of our central and northern fields.

That, therefore, which best indicates the relation between the coal-fields of the south-west of England and those of the north of France and of Belgium, is the similarity of mass and structure, uniformity of subjection to like physical causes, and identity of relation to the underlying older and to the overlying newer formations.

These physical features are of much importance and interest. Of the underground prolongation of the axis of the Ardennes through the south of England there can be little doubt; nor can there be much doubt that the same great contortions of the strata which in Belgium folded alike the Coal Measures, the Mountain Limestone, Devonian, and Silurian series, and were

the cause of similar folds in the same rocks of Somerset and Wales, were continued along the whole line of disturbance, and that the preservation of portions of the same great supplementary coal trough is to be looked for underground in the intermediate area, just as they exist above ground in the proved area. The intermediate subordinate barriers dividing the coal-basins can, I conceive, in no way permanently affect the great major disturbance, by which the presence of the coal measures is ruled.

Admitting, however, that the coal measures were originally present, the question has been mooted whether they have been removed by subsequent denudation.

It has been urged that the Coal Measures become unproductive, and thin out under the Chalk, as they range from Valenciennes towards Calais, and, therefore, that the coal-trough or basin ends there. It is perfectly true that the Coal Measures do thin out between Béthune and Calais, but not in the sense of their dying out owing to their deposition near the edge of a basin. In that case, each seam, each stratum would gradually become thinner and disappear, but such is not the fact. None of the beds of the Belgian coal-field are thick; the average does not exceed $2\frac{1}{4}$ft. At Valenciennes it is the same; whereas M. Burat states that the mean thickness of the beds actually increases westward of Béthune to more than $2\frac{1}{2}$ft. With respect, also, to the extreme end of this basin, the lower beds there brought up correspond with the bottom beds of the Hainaut basin, where the lower 650 feet consist altogether of unproductive measures. The thinning-out is, in fact, due to denundation, just as the Bristol coal-field thins out at Cromhall to resume in the Forest of Dean, or the coal-field of Liège thins out at Nameche to resume at Namur in the great fields of Charleroi and Mons.

The deterioration of the coal in the small coal-field of Hardinghen, near Boulogne, has also been adduced against the occurrence of workable coal in south-eastern England; but Mr. Godwin-Austen has shown that this Hardinghen coal-field is one of those small local developments of coal-bearing strata intercalated in the Mountain Limestone, and is of older date than the great Belgian coal-field. It has, therefore, no bearing on this part of the question.

Another objection, to which much weight has been attached, is, that as the coal-field of Bath and Bristol forms an independent basin, cut off both on the east and on the west by ridges of Millstone Grit and Mountain Limestone, we have there reached the eastern boundary of the coal measures. It is probable that such a bounding ridge does exist, though, as the edge of the basin is there covered by secondary rocks, there is

some uncertainty about the disposition of the palæozoic rocks under them. Admitting, however, the basin to be complete and isolated, that is no proof that the older Palæozoic rocks prevail exclusively to the east; for the coal measures of the Somerset basin maintain their full development to the edge of the basin, and are there cut off by denudation, and not brought to an end by thinning out. They form part of a more extended mass, of which we have there one fragment, while on the west another portion exists in the Welsh basin, and another in the newly-discovered small basin of the Severn valley; and there is no reason why on the east the same disposition should not prevail.

The Severn Valley basin is entirely covered by the New Red Sandstone; and as the Welsh basin is bounded on the east and the Bristol basin on the west by Mountain Limestone, the same objective argument might have been used in either case to show the impossibility of Coal Measures occurring in this intermediate area, or of their extending beyond the boundaries of either great basin.

But the fact is, it is the very nature of the great line of disturbance to have minor rolls and flexures of the strata at, or nearly at, right angles to it, and so causing breaks in the coal-trough, which would otherwise flank it without interruption; thus the Aix-la-Chapelle coal-field is separated by older rocks from that of Liège, which is again separated by a ridge of Mountain Limestone from that of Hainaut. So, in the case of south-western England, we have the separate basins of South Wales, Severn Valley, and Bristol—the extremes of the intervening belts of older rocks being two miles at Nameche and eighteen miles in Wales. These barriers are clearly only local; and the division of the coal measures into separate basins appears to be their ordinary condition along this great line of disturbance. The length of the two known portions of the axis included between Pembrokeshire and Frome, and between Calais and Westphalia, is 472 miles; and in this distance we find eight separate and distinct coal-fields. The combined length of these eight coal-fields is about 350 miles, leaving about 122 miles occupied by intervening tracts of older rocks; so that nearly three quarters of the whole length is occupied by coal strata. I consider that a structure which is constant above ground, so far as the axis of disturbance can be traced, is, in all probability, continued underground in connection with the range of the same line of disturbance; and I see no reason why the coal-strata should not occupy as great a proportional length and breadth in the underground and unknown as in the above-ground and explored area. It is certain the basin-shaped form of the Somerset coal field is no reason why other coal basins,

fragments of the same great original trough, should not exist underground between Somerset and the north of France and Belgium.

With respect to the possibility of denudation having removed the intervening Coal Measures, enormous as the extent of denudation must have been previously to the deposition of the Permian strata, we cannot admit its exceptional action in this case. Denudation has removed from the crest of the Mendips a mass of strata possibly equal to two miles or more in height, and from that of the Ardennes as much as three or four miles; and it has also worn extensive channels between many of our coal-fields; so that the power of such an agent cannot be denied. (See Sect. Plate LXXXV.). But it is a power of planing down exposed surfaces rather than of excavating very deep troughs. Notwithstanding the extent of its action on the Mendips and Ardennes, deep troughs of coal measures are left flanking their northern slopes. These troughs descend to more than a mile beneath the level of the sea; and I do not think it probable that the intermediate underground portions of the trough through South Eastern England, where the axis lies lower, have suffered more than those on the higher levels, except to the extent caused by the later denudation which preceded the Cretaceous period. But this would not affect the main bulk of the coal measures. The Belgian coal-field, which was exposed to the action of both these denudations, still retains vast proportions.

At the same time the pre-cretaceous denudation was very irregular in its action, giving rise to hills and plains. At one place near Mons the chalk and tertiary strata are above 900 ft. thick; whilst at another, on about the same level, and only a few miles distant, they are not 100 ft. thick—an old underground hill of highly inclined coal measures rising in the midst of the unconformable newer strata, and giving rise to this difference. This shows that in the English chalk area we may possibly find irregular old surfaces of this kind, so that the coal measures may exist at places nearer the surface than we have estimated.

We have alluded before to the great length and small width of the Belgian coal-fields. That of Liège is forty-five miles long, with a mean width of less than four miles, whilst that of Hainaut and Valenciennes, with a width scarcely greater, is 119 miles long. The presence of lower carboniferous rocks so far north as Harwich, and the extent of north range of the Bristol coal-field, render it possible that the coal trough in the intermediate area may have a greater expansion than in Belgium; but we have nothing else to guide us, unless it be that the lateral pressure in the intermediate ground was less

than in the Ardennes and the Mendips, where it has exercised its maximum elevatory force. In that case the coal trough in this intermediate area would be less compressed and more expanded, and we might consequently look to find larger coal-basins than those of either Somerset or Liège.

The strata on the south side of the Liège coal-field rise abruptly against highly inclined and faulted Devonian rocks, and on the north side they rise, at a less angle, beneath Cretaceous or Tertiary strata; and to the westward the great palæozoic axis of the Ardennes, consisting of Silurian and Devonian rocks, Mountain Limestone, and Coal Measures, passes westward under the Chalk of the north of France, and has been followed underground as far as Calais, where it lies at a a depth of 1,032 ft.; while in the direction of Boulogne the old rocks keep nearer the surface, crop out from beneath the chalk downs surrounding the Boulonnais, and disappear near the channel under an unconformable series of Jurassic and Wealden strata.

We may, I think, look for a prolongation of this old palæozoic surface of highly inclined, contorted, and faulted rocks at no great depth under the same Cretaceous and Tertiary area of the south-east of England. For, although the old palæozoic surface descends rapidly from 200 ft. above the sea-level in the Boulonnais to 1,032 ft. below it at Calais, it rises at Ostend 47 ft. higher than at Calais, and, crossing the Channel, it is found at Harwich within a few feet of the same depth as at Calais, from which it is eighty miles distant in a northerly direction. Passing westward, we find the palæozoic rocks under London, 105 miles distant from, and 102 feet higher than under Calais, and 106 feet higher than at Harwich. Allowing for irregularities of the old surface as evinced by the well at Crossness, near Plumstead, which was still in the Gault at a depth of 944 feet, or some 14 feet below the level of the palæozoic rocks at Kentish Town, we may still consider that in the area between these three points, and other parts on the same range of the south-east of England, the palæozoic rocks will probably be found not to be more than from 1,000 to 1,200 ft. beneath the sea-level.

Projecting the line another 100 miles westward, we reach the neighbourhood of Bath and Frome, where the coal measures are (as before mentioned) lost, at a depth of about 500 feet, beneath Liassic and Jurassic strata. In the intermediate area between that place and London no trial-pits and no wells have been carried to a depth of anything like 1,000 feet beneath the sea-level. The deepest well with which I am acquainted is one near Chobham, in Surrey, through tertiary strata and chalk to a depth of about 800 feet, or 600 feet beneath the sea-level.

There are, however, in all this area certain indications of the proximity of old land and of pre-cretaceous denudation, in the presence of Quartz and Lydian-stone pebbles, accompanied by extraneous secondary fossils in the Lower Greensands of Surrey, and in the like old-rock pebbles, with the addition of slate-pebbles, in that formation in north Wiltshire; while the banks of shingle, bryozoa, and sponges of the same age at Faringdon, point to still and sheltered waters, probably of no great depth, and to adjacent dry land. Again, on the north of London, we have in the Lower Greensand of Buckinghamshire and Bedfordshire shingle-beds consisting almost entirely of fossils derived from Jurassic strata, with a remarkable collection of larger quartz, quartzite, and other rock pebbles, probably from the old palæozoic axis, which at first stood out in the midst of the Lower Secondary seas, and was only finally submerged in the seas of the Gault and Chalk periods. It was no doubt owing to the gradual shallowing of the old seas as they approached the then palæozoic land that the thinning out of the Lower Secondary rocks from the north-west to the south-east, which we before noticed, is owing.

In this country the newer strata, overlying the palæozoic rocks on parts of the presumed old palæozoic range, have been sunk into without result—in the Wealden at Hastings to a depth of 486 feet, in the upper beds of the same at Earlswood, near Reigate, to about 900 feet, through chalk at Chichester, to 945 feet, and at Southampton, through Tertiary strata and chalk, to a depth of 1,317 feet. Unfortunately all these works fall short of the mark which we as geologists wish to attain.

In a scientific point of view, no experiments could have greater interest, and in an industrial point of view no experiments could be more important, than such as would serve to determine the position of this great underground range of older rocks connecting the Ardennes and the Mendips. We have ascertained that it lies at no great depth beneath the overlying newer strata, and if the strike of the line of disturbance were in a straight line, we should have no difficulty in determining its course; but from what we know of its range in the proved part of the 800 miles, it is certain that while it has a general east and west bearing, it is subject to considerable local deflections. Thus, while Mr. Godwin-Austen is disposed to place the supposed coal trough in the Valley of the Thames or under the North Downs, I am disposed to place it further north, in Essex and Hertfordshire; and while my friend considers it continuous, I consider it to be most likely broken up into basins. Again, if the axis of the Ardennes consisted of an anticlinal line, the problem would be simplified, but it consists of a series of such parallel lines, and therefore whether or not the one which

traverses the Boulonnais and is probably prolonged under our Wealden area, is one of the many central ones, or the lateral one immediately flanking the coal trough, is uncertain.

Any attempt made to solve this great problem must be hailed with satisfaction, and we therefore look upon the trial about to be made by the Sub-Wealden Exploration Committee as a most important step in this direction. The site selected for the experiment near Battle is on the line of centre of the Weald, where the lowest Wealden beds come to the surface, and it is no doubt on or near the line of continuation of the axis of the Boulonnais. We may, therefore, there expect to meet with a prolongation of that axis consisting either of the Mountain Limestone, with the subordinate coal strata of Hardinghen, or the Devonian Limestone and grits. Had it been merely a trial for coal another site might have been selected, but the object the Committee has in view is one purely scientific, to determine the thickness of the Wealden and underlying secondary strata, and the depth, nature and position of the underlying palæozoic rocks; and it has been suggested and planned in honour of the next meeting of the British Association at Brighton —of which meeting it will be a worthy memorial. It is not by one experiment, however, but rather by several, that the important question of the line of the great trough of productive Coal Measures will be determined. This experiment will probably be but one of a series. We hope to learn by it the direction of the strike of the older palæozoic rocks, and then judge of the bearing and probably position of the coal trough in relation thereto. No insurmountable difficulties present themselves. The starting-point is the lowest in the series of the known rocks of the south-east of England, and if it should prove to be on the crest of the prolonged ridge of the Boulonnais, the other secondary strata may be of no great thickness. But this is entirely a matter of experiment. We know not what may be the thickness of the remainder of the Wealden, and if beneath that we find the Purbeck and Portland beds, the Kimmeridge clay, and some of the Oolitic series in place, as we do in the Boulonnais, we know not what thickness they may attain.*

* The thickness of the strata may vary within the following limits:

	Probable Minimum Feet.	Probable Maximum Feet.
Asburnham Beds	50	50
Unproved Wealden	100	300
Purbeck and Portland	30	50
Kimmeridge Clay	300	800
Oxford Clay	200	400
Oolites	50	100
	730	1700

PALÆOZOIC STRATA to be proved beneath these beds.

The Committee wisely contemplate the possibility of having to go to a depth of 1,500 to 2,000 feet, and will, we trust, take secure steps against such a contingency. At the same time we hope (and it is perfectly possible) that before they reach half that depth, the very interesting object of which they are in search may meet its solution, and that one more positive fact may be acquired to science in addition to those so fortunately furnished by the trial works at Kentish Town and Harwich.

BUD VARIATION.

By MAXWELL T. MASTERS, M.D., F.R.S.

THE reproduction of plants is effected in one of two ways, either by the contact of one elementary organism with another of a different kind, in consequence of which a spore or an embryo is formed, which ultimately developes into a perfect plant, or by the production of buds.

The word bud is here used in a broad sense to express any separable portion of a plant, not produced by sexual agency, and which when separated has the power of growing into an organism like the parent plant. The process of bud-formation then, reduced to its simplest expression, is a process of segmentation, or subdivision. Illustrations are to be found throughout the whole vegetable kingdom, but in no family are they more frequent, or do they play a more important part, than in the great group of the Fungi, among which are the moulds and blights so destructive to the higher plants on which they grow. One of the most remarkable circumstances about these plants is the varied manner in which they are reproduced. Spores, or reproductive bodies of four, five, or more shapes are met with at different times on the same plant, and, inasmuch as they are often formed at various times and under diverse conditions, it is no matter for surprise that they should have been assigned, not to the same plant, but to different ones, and hence each one has had the misfortune of being separately named.* Now, thanks to the labours of those who have, with infinite skill and patience, succeeded in unravelling the life-history of these plants, all these varied forms are known to be different states of the same plant. Of these spores some are true reproductive bodies in the sense already explained, while others are buds extending and multiplying the plant, but not reproducing it.

We do not know in all cases, indeed we only know in a few,

* See a paper on the subject of Polymorphism in Fungi, in "Popular Science Review," Jan. 1871, y Mr. M. C. Cooke.

the complete life history of these plants and the particular office the bud-like formations fulfil. For our present purpose, however, it will suffice to say that they vary in size, form, and apparently in the conditions under which they are produced. In spite of these diversities, we know that they develop into organisms precisely like those from which they sprung.

Among the sea-weeds the same state of affairs exists; there are true spores and bud-spores, and these bud-spores vary in character on the same plant at different times and in different seasons.

In the Lichens we have a similar formation of true spores and bud-spores, but so far as is at present known, there is not the same diversity in the bud-spores, or "gonidia," of Lichens that there is in the other groups. There is, however, this difference; the bud-spore of the lower plants consists of a single cell, whereas in the Lichens it is made up of several cells: it is an aggregate, not a unit.

In Hepaticæ and Mosses the bud-spores are like those of Lichens, but more highly-organised. In the case of the Ferns and Equiseta there are buds very nearly like those of flowering plants, consisting of a number of minute scales, the outer of which remain scaly, and ultimately perish; the inner gradually develop into leaves, while the central pimple of cellular tissue from which these scales emerge lengthens into a shoot, that shoot into a branch, and so on.

Moreover, that bud, if separated and placed under proper conditions, will form a new plant.

In this way the gardener prepares his cuttings. He takes a "slip" with a bud attached, places it in moist earth, covers it with a bell-glass to prevent undue evaporation, and places it in a sufficiently warm locality. After a time the cutting "strikes," as it is termed; that is, it forms roots, which roots absorb nourishment. The cutting is thus truly a chip of the old block. That which the gardener does by art Nature herself often does unassisted. Many Begonias form buds from almost any portion of their surface, and in prodigious numbers, recalling the way in which similar buds are formed on the Mosses, but in even greater profusion. Other illustrations may be seen in the little bulbs which beset the stalk of the tiger-lily, or protrude from the margin of the leaf in *Bryophyllum*. This process of bud formation occurs also, to some extent, in the animal kingdom, as among the hydras, but is by no means of such general occurrence as in plants.

Under ordinary circumstances all the buds on any particular plant are in all material points alike, and the shoots resulting from those buds are also alike. There are differences in size and vigour and what not, for no two are precisely alike any

more than any two sheep in a flock, or any two peas in a pod, are precisely alike; still, for general purposes, we may say that all the buds and all the shoots from those buds are alike. To such an extent is this true that it is the general practice amongst gardeners to propagate, by means of cuttings or grafts, any particular variety they may be desirous of perpetuating, because reproduction by seed does not offer the same certainty of reproducing the particular quality required as propagation by buds does. But it now and then happens that one or more buds on a particular plant, and one or more shoots, are not like the rest, and then we have what in garden phraseology is termed a "sport," but which is more correctly styled a bud-variation.

We propose to cite sundry selected illustrations of this phenomenon, with a view to show how wide the range of variation may be, and in what different ways it may manifest itself. The simplest case, because it involves no appreciable change of form, is that in which a single bud, or a collection of buds in one particular part of a plant, is more precocious in its development than the others on the same tree. Instances of this kind are not uncommon. The buds on one particular branch may be each year considerably in advance in point of development of their neighbours, and this without there being any appreciable reason, such as more perfect protection or shelter on one side than on another. Thus we have seen two shoots of red currants taken from the same branch: on the one spray the flowers were ten days earlier in point of expansion, the new shoots being as much as 6 in. in length, while on the other spray the buds were only just expanding. With reference to this point, it may be remarked that the same phenomenon occurs in the case of seedling varieties. There are certain horse-chestnuts—some of which have almost historical fame, such as the Marronnier du Vingt-Mars in the Tuileries Gardens—which are year by year several days in advance of their kind in their development. But the circumstance of the whole organism exhibiting this precocity is not so striking as is the early development of one particular branch or set of branches, as compared with the rest.

In point of size, whether increased or diminished, there is often great difference in the different branches of the same tree. For some reason or other—what, no one knows—the shoots on a particular branch, instead of lengthening as the rest do, remain stunted and dwarfed. Several curious garden varieties of firs, such as the Clanbrasilian fir, have originated in this way, and are reproduced or propagated by cuttings or grafts at the will of the gardener. The birch affords frequently illustrations of this phenomenon, in the form of those tufted agglomerations

of contracted shoots so strikingly resembling birds' nests. A similar occurrence is not uncommon in the wild cherry; and a correspondent—Mr. Webster, of the gardens, Gordon Castle—informs us that he has observed similar growths in the common laburnum, in the Wych elm, and in the Scotch fir. Sometimes the determining cause may be discovered in the shape of an insect or fungus, but in this case the unusual condition ceases with the destruction of the impeding cause, whatever it may be, and the condition cannot then be perpetuated by the art of the gardener.

Variation in the colour of certain leaves or flowers is an equally common occurrence, and is perhaps more easily understood. Each individual cell, to a large extent, lives independently of its neighbours, and the secretions it forms and deposits are very often different from those of adjoining cells. Colouring materials, especially fluid ones, are notoriously liable to be formed in isolated cells. Again, variations in colour so often depend on the mere superposition of cells containing material of different tints, that the changes met with, though striking to the eye, do not seem to indicate so complete a change as in the case of alterations of form or size. Very many of the variegated Pelargoniums, so fashionable now-a-days, have originated as "sports" from some previously existing variety. The intrinsic change between some of these varieties, even where apparently very considerable differences exist, is, in some instances, very slight.

A marked difference in the amount and quality of the pubescence is not unfrequently manifested in some of these cases of bud variation. A plant which ordinarily has its leaves and its younger branches invested with a coating of hairs (epidermal appendages), all on a sudden produces a shoot on which the leaves are destitute of such clothing, or *vice versâ*. Some of the moss roses have originated from plain-leaved varieties in the manner just indicated.

But of all these cases the most striking are those which involve a change of form. We see, for instance, not unfrequently a particular branch bearing leaves very different from those on the rest of the tree, so different that but for their production on one and the same tree, the observer might readily take them to belong to different species. Many trees now cultivated for ornamental purposes have originated in this manner, such as the cut-leaved beech, the oak-leaved laburnum, and very many more, commonly to be found in plantations. Very often the whole "habit" or aspect of the tree is altered by these variations: thus many of the so-called "weeping trees" have sprung from a solitary branch of a tree which presented a pendulous character. Some trees, it may be remarked, naturally produce

leaves of very different forms: especially notable in this respect is the Euphrates Poplar, *Populus euphratica*, supposed with reason to be the willow mentioned in the Psalms. Occasionally the variation is confined to one half of the leaves. A remarkable instance of this kind has been noted by A. Braun in a species of *Irina*, where one half of the leaf was undivided, the other deeply gashed into narrow segments.

The history of these variations is pretty much the same in all cases. All on a sudden a tree, which heretofore has produced shoots and leaves of the usual character, emits shoots with leaves of a totally different form. If they be such as the cultivator thinks likely to serve his purpose, he takes care to propagate them by means of grafts or cuttings. Sometimes variations of this character may be reproduced by seed, but there is little certainty as to this. The same kind of variation occurs in flowers and fruits. In the former it is usually associated with distinctly recognisable alterations in the phenomena of reproduction, as in what are spoken of as dimorphic or trimorphic flowers, some instances of which have been so carefully investigated by Mr. Darwin. To this latter class of bud variation we shall do no more than make passing allusion, but there are other cases which have apparently no relation to variations in the phenomena of fertilisation or reproduction, and which are strictly analogous to those already mentioned as occurring in the leaves. Every now and then, for instance, two roses of different forms and colours will be met with on the same stalk, such as a white moss rose in association with a pink one of a different form and destitute of mossy appendages. We have in a former paper in this Journal referred to some of these cases and to the famous *Cytisus Adami*—a laburnum bearing yellow and purple flowers as well as leaves of different character—and have also alluded to the alleged causes of these strange phenomena, on which account it is not necessary now to do more than refer to them. What is a rare occurrence in the rose, and is only known in one or two species of laburnum, is comparatively common in the chrysanthemum. There are indeed particular varieties of this favourite autumn flower which are specially liable to produce flowers of different characters on the same branch. Generally speaking, but by no means always, the change is confined to the colour of the flower only, and colour, as we have seen, is proverbially fickle in flowers. Among commonly cultivated plants azaleas and camellias are peculiarly liable to "sport." In the former plants indeed one may often witness much variation in the shape and colour of individual blossoms, and very frequently parti-coloured flowers and others intermediate between extreme forms. In the case of the fruit similar variations occur—peaches and nectarines on the same

bough ; black and white grapes in the same bunch ; gooseberries of different kinds on the same bush ; pears, apples, or cherries, of different shapes, colour, and flavour, on the same bough. All these are, though of course rare, yet familiar occurrences to those on the look-out for such phenomena. It is necessary in some of these cases to investigate closely to see whether or no grafting of different sorts on one stock has not taken place. No doubt some of these cases, recorded by lovers of the marvellous, were simply cases of adhesion or inoculation, but, allowing for these, there still remains a large number which cannot be explained by any such process.

The above-cited illustrations might be largely added to were it necessary to do so. Mr. Darwin's work on "Animals and Plants" contains allusions to many others, and includes many references to the literature of the subject. The horticultural journals, British as well as foreign, contain very numerous records of such cases; * but we have cited enough for our present purpose, and may now pass on to the discussion of some of the alleged causes of the phenomena in question.

It must first of all be premised that these bud variations are not necessarily to be considered as malformations. Their organisation is often perfect, they are not distorted, they are simply variations ; and next, they occur not exclusively in plants that have been long subjected to cultivation, but also in wild plants. Now plants that have been long in cultivation have for the most part been hybridised or "crossed" over and over again. Thus in the case of the pelargonium, it is supposed that all the immense number of different kinds now in cultivation have originated from two or three species. These have been hybridised or crossed, their offspring has been crossed in the same way, and so in the pelargonium of the present day we have a plant which has, so to speak, a great deal of very confusedly mixed blood in it.

Bud variation is very often only a reversion—a harking back—to the characters possessed by the parent; it is the result, as the phrase goes, of a *dissociation of hybrid characters*, the consequence of a sort of filtration by which the constituent elements become separated from their previous admixture.† This reversion may be proximate, just as you may see in a family of children that, while most of them resemble both

* A list of many such instances may also be found in M. Carrière's "Production et Fixation des Variétés."

† The papers of Naudin, Braun, Rejuvenescence (Cytisus Adami), and Duchartre, Note sur le Chasselas Panaché, in the "Journal de la Société impériale et centrale d'Horticulture," 1865, should be read in reference to this part of our subject.

parents, some are like the one or the other, while some again present little likeness to either parent, but reproduce the lineament of some remote ancestor. A singular illustration of this phenomenon was brought under the writer's notice by Mr. Wills, and in which two plants of pelargonium showed the characters of three separate ancestors; the exact lineage of one was not fully known, but the history of the other was definitely recorded. The plant in question presented, after the fifth generation by seed (and not till then), various branches bearing leaves undistinguishable from those of the varieties known as "Unique," "Beauty of Oulton," and "Italia Unita"—three very distinct varieties, each of which were known to have been at some time or another ancestors of the plant in question, either as furnishing pollen or as the seed-parent.

Another plant of mixed origin, after retaining its characters for three years, suddenly produced branches some of which had leaves of the form and colouration of those of "Beauty of Oulton," the original seed-parent, while the remainder were bedecked with leaves in all respects similar to those of "Lucy Grieve," the ancestral pollen or male parent. The two varieties in question are widely different. In the cases just alluded to there was not a mere change of colour—an affair of comparatively minor importance—but there was a change of configuration and substance. Other cases of a similar nature have been recorded by various observers, amongst others by Mr. Grieve, the raiser of the popular "Mrs. Pollock" pelargonium.

Of course any plant produced from seed requiring for its development the contact of the pollen tube with the ovule or the germinal vesicle must be held to have mixed characters, and more markedly so in the case of unisexual flowers, either monœcious or diœcious. From this point of view a case lately recorded by Mr. Meehan becomes very significant. That gentleman relates that he obtained cuttings from *Cuphea leiantha*, a diœcious plant, producing its male and female flowers on different individuals. It is not stated whether the cuttings were taken from a male or a female plant, but it is stated that some of these cuttings produced male, others female, plants, and yet all were taken from a plant of one sex only. So, too, it is well known that certain unisexual trees will in some seasons produce male flowers only, in other seasons female flowers only, and *vice versâ*.

To enter into questions relating to the sexuality of plants would, however, lead us too far. We merely now indicate the facts, as proofs of the composite character of the plant.

But dissociation of mixed characters will not account for all the cases of bud variation. Very often we have no evidence at

all of previous hybridisation or crossing; or, even where such has existed, the form produced is not like that of either of the supposed progenitors.

Such cases as the fern-leaved beech do not seem explicable by either hypothesis. The sugar-cane, which rarely if ever flowers, and hence offers no opportunity for hybridisation, nevertheless produces new varieties by means of bud variation. Potato tubers, again, vary greatly often on the same plant, but these may be the result of former crossing. A case related by Mr. Meehan, in the sweet potato (*Convolvulus Batatas*), is, however, not open to this objection. The plant in question, it appears, never flowers in the Northern States of America, and yet it has been known to produce tubers of two distinct varieties—the "Red Bermuda" and the "White Brazilian"—on the same root.

Reversion to an ancestral condition is a still more hypothetical cause than dissociation of mixed characters, as we have scarcely ever any means of knowing what the assumed condition was. We have, therefore, to look to other causes. We shall not advance matters much by attributing the changes in question to an innate tendency to vary possessed by buds as well as by seedling plants, which are, in so many respects, analogous with buds. Doubtless there is such a tendency, but we want to get at the "why and wherefore" of the proclivity. The following illustrations may in some slight degree furnish a clue to the attainment of the desired end. In the first place we must not overlook the circumstance that, under ordinary conditions, the several organs of plants often vary according to the part of the plant upon which they grow. Botanists recognise this when they give different names to the root-leaves, stem-leaves, floral-leaves, bracts, &c. Again, there are such cases as the seedless barberry. This plant can be propagated by cuttings, and its seedless condition can be thus perpetuated; but if the plant be multiplied by suckers or shoots thrown up from the underground stem, the fruits produced have seeds as usual. This is an evidence of a difference in the internal organisation of different parts of the same plant. Another illustration of a similar character lately came under observation, in which a sucker from the root of the tree of heaven, *Ailanthus glandulosa*, produced egg-shaped leaves and a dense cluster of flowers while only a foot or so in height (see fig. 1), the ordinary habit of the tree being to grow for several years before flowering, to form a lofty stem, and to produce large compound pinnate leaves like those of the common ash. This, in gardening phrase, would be a "sport," but it is clear it had nothing to do with hybridisation, the form produced being unlike that of any other allied plant. Moreover, there is no

evidence to render the occurrence of hybridisation in this particular case at all probable. We can only attribute it to a

Fig. 1.

difference in the organising force manifested in certain parts of the plant as contrasted with others.

Of a similar character are the observations made by practical gardeners as to the difficulty, and in some cases impossibility, of perpetuating a variegated condition of the leaves by dividing the root; plants so produced having green leaves. A French nurseryman, M. Lemoine, notes this in the case of variegated pelargoniums, and in certain forms of *Symphytum* and *Phlox*, and his experience tallies with that of English cultivators. Again, in the common practice of budding roses, if the bud be taken from a long rampant "gross" shoot, with a great tendency to form leaves and little tendency to produce flowers, the bud, transferred to its new home, will reproduce the undesirable characters of the parent shoot: hence the care requisite in budding to take buds from short-jointed flower-bearing shoots.

A similar precaution is exercised by gardeners in the case of fruit-tree grafts.

The different forms which plants assume at different stages of their existence under normal circumstances must also be taken into consideration in speculating on the origin of bud variation. A large number of plants do not immediately assume their wonted habit, they pass through an intermediate stage or stages. This is particularly observable in the case of Conifers, the juvenile state of which is often very different from the appearance presented in the adult state. It now and then happens that, after a plant has lost its youthful characters and assumed its full-grown developement, sundry branches, for some unknown reason, revert to the infantile form. In the common ivy we have a familiar illustration of a similar phenomenon. When the plant is about to produce flowers it assumes an erect bushy habit, its leaves alter in form, indeed its whole aspect becomes changed. If now such branches be taken off and propagated, the characteristic form remains as in what are called tree ivies. If the life history of such a plant were not known, the bud variation just mentioned would appear even more inexplicable than it now does. Again, the leaves and flowers produced on the same plant at different seasons are often naturally different. Dr. Balfour has lately called attention to a remarkable instance of this phenomenon in a species of hawkweed, *Hieracium*, which presents three distinct forms according to the season at which it flowers. Occasionally even a casual observer is struck by the appearance of a second or even a third crop of flowers on laburnums, or pear-trees, Wistarias, and others. In such instances it will generally be found on examination that the adventitious flowers spring from buds which under ordinary circumstances never produce flowers, but only leaves, or that they are placed on portions of the tree usually devoted solely to the production of leaves. How much the aspect of the tree is altered in such cases may readily be surmised: the casual spectator cannot fail to notice it, but the explanation of the phenomenon rarely strikes him.

As might have been anticipated, a change in the external conditions under which a plant lives will often cause very considerable variation in its form: thus a species of fig, *Ficus stipulata*, is commonly grown on the walls of hot-houses, to which it clings ivy-fashion. The same plant grown as a standard in a pot has a totally different appearance. On the wall it has small thin leaves, and it clings to its surface like a large moss or a miniature ivy. Planted out it forms a stout bushy shrub with large coarse leathery leaves, so different from those formed when the plant is growing against a wall that no botanist unacquainted with the history of the plant would

hesitate for a moment in ranking it as a distinct species. Some of the Marcgraavias present similar phenomena. In both the plants just named the writer has seen on plants growing against a wall shoots produced of the character of those formed by the plant when growing unsupported. The inference from these facts is that what we call "sports" or bud variations are often only exceptional illustrations of a normal tendency—exceptional in so far that they are manifested at unusual times and places and under unusual conditions.

The individuality or comparative independence of buds—a circumstance often noticed—is also brought prominently into view by such facts as we have recorded. An interesting question arises as to whether there are differences in the plant originating from bud variation as compared with one the produce of variation from seed. It is a matter of every-day experience with gardeners that seedling plants vary greatly—even though the produce of the same seed-vessel, and even though not the offspring of hybridized or cross-fertilised parents. Is there any perceptible difference between a seedling variety obtained as just explained and a bud variety? In other words, are there any means of distinguishing, in the case of a cultivated plant of unknown history, a "sport" from a "seedling"? We have tried in vain to find any such difference. The experience of the most able cultivators furnishes no data on this head. But although this is so, there is an equally prevalent impression that while a variety cannot always be perpetuated "true" from seed, it can be propagated unchanged by cuttings or grafts. The best varieties of apples or pears—to cite only one instance—are propagated by grafts, because there is no certainty at all that the pips will reproduce the desired variety; far more commonly they produce something else. There is, then, a difference between seed variation and bud variation, in the greater degree of permanence of the latter. That this difference is not absolute is shown by the following case recorded by M. Rafarin in a French horticultural journal.

"In 1866, at La Muette, a pelargonium with pale rose-coloured flowers was observed to bear a branch, all the flowers on which were of a deep red colour. Cuttings were taken from this 'sport,' from which 20 plants were raised, which flowered in 1867, when it was found that scarcely two were alike. Thus while some bore rose-coloured flowers like those of the original plant, others had red flowers, like those of the 'sport'; others again had red and rose-coloured blossoms on the same plant and even in the same truss. Nay more, even the petals partook of the parti-coloured nature, for in the same flower were petals of a rose, or a red colour, or of a blended hue. Unfortunately neither the name of the variety nor its genealogy are given, so

that we are unable to say positively whether this was a case of reversion or no."

It may perhaps be said that seedling variations such as happen in the apple or the pear are the necessary outcome of the cross-fertilisation to which the plants in question have been subjected for ages, just as the bud variations in the case of the pelargonium are. This may be true in some cases, but can hardly be so in all: for instance, in a bed of seedling conifers, such as Lawson's cypress or deodars, raised from imported seeds taken from wild plants, often from the same cone, the number of seedling varieties is often large. In the case of cultivated plants as of domestic animals, Mr. Darwin has shown how the variations that arise are directly connected with the objects for which the particular plant or animal is cultivated. A plant, for instance, grown for the sake of its fruit is apt to vary in its fruit characters more than in its leaf characters. But although this may and no doubt does apply to a considerable extent in the case of seminal variations, it seems less applicable in the case of bud variations, as will be judged from the illustrations before given, as also from the negative evidence afforded by a plant like the Jerusalem artichoke, which is propagated exclusively by its tubers, and indeed never ripens its seed in this country, and which has produced no variation by "sport" or dimorphism, although so largely grown and for so long a period.

Mr. Darwin attempts to explain the phenomena of bud variation, as of inheritance and reproduction generally, by his hypothesis of pangenesis. This hypothesis proceeds on the assumption that every cell of a living organism gives origin to an innumerable host of "gemmules" in minuteness as in number transcending conception. These gemmules divide and multiply, or they lie dormant possibly for ages. They circulate throughout the organism or they become aggregated together, and so form embryos or buds, and they are transmitted from one generation to another. There is nothing improbable in the assumption of the existence of these gemmules; and, if we take their presence for granted, it is easy to see how they afford an explanation of the phenomenon of reversion to an ancestral condition, such as bud variation so frequently presents. Gemmules derived from a plant's remote progenitors are, according to the hypothesis, circulating in the present generation, and it only requires the occurrence of favourable conditions to determine the revivification of these now dormant gemmules to reproduce the ancestral form. There still remains the difficulty of ascertaining what the favourable conditions are which determine this change. The reason for the prolonged dormancy of the gemmules is also not obvious. But, supposing we admit the gemmule hypothesis as sufficient

to account for reversion by bud variation, there yet remain that larger class of bud variations wherein there is no suspicion of reversion.

This latter category, so far as we see, can only be explained by Mr. Darwin's assertion that, in "cases in which the organisation has been modified by changed conditions, the increased use or disuse of parts or any other cause, the gemmules cast off from the modified units of the body will be themselves modified, and, when sufficiently multiplied, will be developed into new and changed structures."

But before we can, with propriety, avail ourselves of this latter explanation, we have to be satisfied that a change of conditions has really been in operation. And this is too often beyond our ken. The majority of bud variations not distinctly referable to reversion, appear suddenly, without any obvious change of external condition, we know not why or wherefore. Suppose we admit, on the ground of intrinsic probability, the operation of changed conditions, even though we may have no direct evidence on the point, we have yet to explain how and why one particular shoot on one particular part of a plant should be acted on in this way, when there is no appreciable reason why it should be influenced more than the rest.

There is still another way of explaining the phenomena on the gemmule hypothesis, and that is, by supposing changes in the number, arrangement, or position of the gemmules; and this supposition, though plausible, is yet based on a number of mere assumptions, and, moreover, it leaves the cause of the altered condition of the gemmules entirely unexplained.

To sum up, then, we may say that there is no absolute difference between bud variation and seed variation. The changes manifest themselves in the same manner and in the same organs in the case of buds or seedlings respectively. The conditions, so far as we know, that produce variation in the one are the same that are effectual in the other. Lastly, apart from the different mode of origin, there is no essential difference between a bud formed as the result of fertilisation, *i.e.*, an embryo, and one formed without the direct agency of the two sexes, *i.e.*, a bud.

AN ACCOUNT OF A GANOID FISH FROM QUEENSLAND (*Ceratodus*).

By DR. ALBERT GÜNTHER, F.R.S.
Assistant Keeper in the Zoological Department of the British Museum.

[PLATE LXXXVI.]

The genus *Ceratodus* has been established by Professor Agassiz for teeth which are found in strata of Jurassic and Triassic formations in various parts of Europe and India. These teeth (fig. 3), of which there is a great variety with regard to general shape and size,* are much longer than broad—sometimes 2 in. long—depressed, with a flat or slightly undulated, always punctuated crown, with one margin convex, and with from three to seven prongs projecting on the opposite margin. They have always been found isolated, sometimes with a portion of a bony base attached to them. Yet Professor Agassiz pointed out, from their shape, that there must have been only two of them in the upper jaw and the same number in the lower, that the convex margin was directed inwards, and the prongs outwards. No other part of the fish to which they belong has hitherto been found associated with them; but Agassiz considered it to have been a cartilaginous fish, or more especially a shark—a view not so very far from the truth, as we shall see hereafter.

The discovery of a Ceratodont fish in the recent fauna is due to the Hon. William Forster and Mr. Gerrard Krefft, the Curator of the Australian Museum at Sydney. Years ago the former of these gentlemen had informed Mr. Krefft that there existed in the fresh waters of Queensland a large fish with cartilaginous backbone, but he was thought to be mistaken

* Mr. Higgins possesses the largest, and probably most unique, collection of Ceratodont teeth from one locality—viz. from Aust-passage near Bristol. Among some 300 specimens there are scarcely two which are sufficiently alike to be assigned to the same individual.

until he succeeded in obtaining for Mr. Krefft a specimen which, although in an imperfect state of preservation, removed all doubts on the matter, and enabled Mr. Krefft to communicate this remarkable discovery to the Zoological Society of London (April 28, 1870). He says: "The discovery of a species of *Lepidosiren* in Australia will, no doubt, take the scientific world by surprise—the more so as this newly-found amphibian has a dentition different from that of *Lepidosiren*, and closely resembling the teeth of certain fossil sharks described by Agassiz under the generic term of *Ceratodus*. On this ground, and being convinced that the various species of animals classed under the name of *Ceratodus* were not sharks, but amphibians, I shall adopt Professor Agassiz's name, and describe the Australian amphibian, in honour of its discoverer, as *Ceratodus forsteri*."

As soon as Mr. Krefft had recognised the importance of this discovery, the Trustees of the Australian Museum took steps to secure well-preserved examples. They sent a collector into the district where the animal was known to occur; and, with their usual liberality, they despatched to the British Museum for examination the first specimens they could spare, by which I was enabled to work out the details of its structure. Some attempts subsequently made to obtain other examples appear to have been unsuccessful, as the fish is locally distributed, and easy of capture at a certain season or at a certain state of the water only.

The specimens hitherto obtained have come from the Burnett, Dawson, and Mary Rivers, some from the fresh water of the upper parts, others from the lower brackish portions. The fish is said to attain to a "weight of twenty pounds," and again to a "length of six feet," the largest example sent to the British Museum being about three and a half feet long. Locally the settlers call it "Flat-head," "Burnett-," or "Dawson-Salmon," and the aborigines "Barramunda," a name which they appear to apply also to another similar fish, the *Osteoglossum leichardti*. I found the intestinal tract crammed full of more or less masticated leaves of various plants (*Myrtaceæ* and *Gramineæ*; they had lost the green colour entirely, being of a uniformly deep black, as if they had lain in water for some time, and were eaten when in a decomposing condition. The quantity of these vegetables is enormous, and there is no doubt that they constitute the principal food of the fish. Shells, fragments of which have been found in the stomach, may have been swallowed accidentally; however, it has been stated repeatedly that the fish can be caught with a hook baited with a worm. The flesh is salmon-coloured, and much esteemed as food.

The Barramunda (we will use this probably oldest name) is said to be in the habit of going on land, or at least on mud-flats; and this assertion appears to be borne out by the fact that it is provided with a lung. On the other hand, we must recollect that a similar belief has been entertained with regard to *Lepidosiren*, of which now numerous examples have been kept in captivity, but none have shown a tendency to leave the water. I think it much more probable that the Barramunda rises now and then to the surface of the water in order to fill its lung with air, and then descends again until the air is so much deoxygenised as to render a renewal of it necessary. It is also said to make a grunting noise, which may be heard at night for some distance. This noise is probably produced by the passage of the air through the œsophagus when it is expelled for the purpose of renewal.* As the Barramunda has perfectly developed gills, besides the lung, we can hardly doubt that, when it is in water of normal composition and sufficiently pure to yield the necessary supply of oxygen, these organs are sufficient for the purpose of breathing, and that the respiratory function rests with them alone. But when the fish is compelled to sojourn in thick, muddy water charged with gases which are the product of decomposing organic matter (and this must be the case very frequently during the droughts which annually exhaust the creeks of tropical Australia), it commences to breathe air with its lung in the way indicated above. If the medium in which it happens to be is perfectly unfit for breathing, the gills cease to have any function; if only in a less degree, the gills may still continue to assist in respiration. The Barramunda, in fact, can breathe by either gills or lungs alone, or by both simultaneously. It is not probable that it lives *freely* out of the water, its limbs being much too flexible for supporting the heavy and unwieldy body, and too feeble generally to be of much use in locomotion on land. However, it is quite possible that it is occasionally compelled to leave the water, although I do not believe that it can exist without it in a lively condition for any length of time.

Of its propagation or development we know nothing except that it deposits a great number of eggs of the size of those of a newt, and enveloped in a gelatinous case. We may infer that the young are provided with external gills, as in the African *Lepidosiren* and *Polypterus*.

Before I proceed to the description of the Barramunda, it

* Gurnards (*Trigla*) and Bull-heads (*Cottus*) are well known to produce a similar noise when drawn out of the water, by the air rushing from the air-bladder through the œsophagus.

will not be out of place to refer here to a remarkable fact in geographical distribution which I have omitted in my previous communications on *Ceratodus*. The division of fresh-water fishes offers not a few instances in which two or more natural families, much differing in their structural characters, have exactly the same geographical distribution. We shall see subsequently that *Ceratodus*, *Protopterus*, and *Lepidosiren*, are members of the same natural Ganoid family (*Sirenidæ*). Now this family coincides in respect of its geographical range with a Teleosteous family which I have called *Osteoglossidæ*, and which comprises the genera *Osteoglossum*, *Arapaima*, and *Heterotis*, as will be seen from the following table:—

Ganoid.	Teleosteous.
Tropical America.	
Lepidosiren paradoxa.	Osteoglossum bicirrhosum. Arapaima gigas.
Tropical Australia.	
Ceratodus Forsteri. Ceratodus miolepis.	Osteoglossum Leichardti.
East Indian Archipelago.	
xx.	Osteoglossum formosum.
Tropical Africa.	
Protopterus annectens.	Heterotis niloticus.

Thus it is only in the East Indian archipelago that we have not yet found the Ganoid representative of the Teleosteous *Osteoglossum formosum*. That it will be found there I have no doubt. *O. formosum* has hitherto been obtained in Sumatra, Banka, and Borneo; and of the inland fishes of the latter island scarcely anything is known at present.

The *body* of the Barramunda (fig. 1) is eel-shaped, but considerably shorter and thicker than a common eel, and covered with very large scales. The *head* is nearly entirely naked, covered with a porous skin, flattened and broad, the eye lateral and small, the mouth in front of the broad snout comparatively narrow, and provided with thick and soft lips. The gill openings are a narrow slit on each side of the head, immediately in front of the fore-paddle. There are no external nostrils. The foremost portion of the trunk is depressed like the head, but it soon passes into the compressed remaining portion, the boundary between trunk and tail being externally indicated by the vent only, which is situated between the hind-paddles. The *tail* varies in length; it is sometimes shortened, and it appears that mutilations of this part, particularly when

happening in early youth, are readily repaired. The tail diminishes rapidly behind in vertical dimension, till it ends in a thin point. The entire tail is surrounded by a broad, vertical fin, which commences on the back behind the middle of the trunk, and is supported by innumerable fine cartilaginous rays. There are two fore and two hind *paddles*, similar to each other in shape and size, and very different from the fins of ordinary fishes. They are covered with small scales along the middle from the root to their extremity, and surrounded by a rayed fringe similar to the vertical fin. These paddles are flexible in every part and in every direction, and too feeble to assist in locomotion on land; they may be of use when the animal crawls in water over the muddy bottom of a creek; but the principal organ for locomotion is the tail, as in tailed Batrachians and the majority of fishes.

The *nasal openings* and the *dentition* can be seen only after the mouth has been slit open. The situation of the former within the cavity of the mouth, two on each side, is a very important character, which hitherto had been known in *Lepidosiren*. The number and form of the teeth has been noticed above, and we have only to add that, beside those molar-like teeth, there are a pair of incisor-like teeth in the fore part of the palate, obliquely inserted in the vomer, and without corresponding teeth in the lower jaw. Knowing the kind of food taken by the Barramunda, we can readily perceive that the incisors will assist in taking up or tearing off leaves, which are then partially masticated between the undulated surfaces of the molars.

With regard to the scales, we may add that a slight difference in their number has been observed in the specimens examined, a difference which, on a more extended examination, may prove to be not constant. The specimens from the Burnett River, to which first the name *Ceratodus forsteri* has been given, have the middle of the trunk surrounded by eighteen series of scales, whilst there are twenty-one of these series in examples from the Mary River. Consequently the scales appear conspicuously smaller and more numerous in the latter form, which I have named *C. miolepis*.

The *skeleton* is cartilaginous; where ossification appears, it is in the form of a more or less thin covering enveloping the cartilaginous substratum, but never taking its origin in the interior or by transmutation of the cartilage. Instead of a vertebral column we find a simple long, tapering chord, without any segmentation, but supporting a considerable number of apophyses. Twenty-seven of them are abdominal and bear well developed ribs. A positively defined boundary between the notochord and the skull does not exist, but in a vertical

section the tapering end of the former may be traced wedged into the basal portion of the skull. The skull is a completely closed cartilaginous capsule, nearly entirely covered with superficial bones, to which, again, some other cartilaginous elements are appended. In the former the confluence of cartilage is so complete that no distinct divisions are traceable by sutures. The tegumentary bones may be designated as, 1, ethmoid; 2, a pair of ossa frontalia; 3, a pair of ossa pterygo-palatina, bearing the upper molars; 4, a single sclero-parietal; 5, an os basale; 6, an os quadratum; 7, operculum; 8, suboperculum; and 9, mandible. Distinct maxillary and inter-maxillary elements are not developed, but replaced by facial cartilages which pass into the suborbital ring.

On the whole the structure of the skeleton reminds us much of that of the Sturgeons, *Chimæra*, and *Lepidosiren*, and of all the modifications by which it differs from these allied fishes none is of greater interest than the peculiar structure of the paddles (fig. 2). The fore-paddle is joined to the scapular arch by an oblong cartilage (forearm) and by a broad basal cartilage (carpus). The central part, which we have found externally to be covered with scales, is supported by a jointed axis of cartilage extending from the carpus to the end of the paddle; each joint bears a pair of three, two, or one-jointed branches. The skeleton of the hind-paddles is formed on exactly the same plan. This singular structure is of interest from several points of view: 1. The analogy of this framework to that of the caudal portion of the vertebral column is obvious, *Ceratodus* being not only truly diphycercal as far as the termination of the body is concerned, but also with regard to the extremity of its paired fins. The many-jointed pectoral axis may be compared to the series of neural and hæmal apophyses, both forming the base to a system of superadded processes (here two or three-jointed branches, there neural and interneural, hæmal and interhæmal spines), which are destined to serve as supports to the surrounding soft parts, and more especially the rays of the fin. Further, as the heterocercal tail of the sturgeon is justly considered to be a later development of the diphycercal form, so the pectoral fin of those fishes proves to be nothing but the heterocercal modification of the diphycercal Ceratodont paddle. 2. The singular filamentary limbs of *Lepidosiren* prove to be typically the same as the paddles of *Ceratodus*, but there they are reduced to the simple central cartilaginous axis, with the addition of only rudimentary rays in the African species. 3. Professor Huxley has already drawn attention to the affinity existing between the limbs of *Lepidosiren* and certain fossil Ganoids, of which impressions f paddles with scaly centres have been preserved (fringed or

lobate fins). *Ceratodus* clearly proves the correctness of this view, and we are fully justified in supposing that those extinct fishes with lobate fins had them provided with a similar internal skeleton.

As in the other Ganoid and Plagiostomous fishes, the *heart* of *Ceratodus* (fig. 4) is provided, in addition to the ordinary two divisions of the fish-heart, with a third contractile chamber, the *conus arteriosus*. The internal structure of the ventricle and atrium is extremely similar to that of *Lepidosiren*, but the valvular arrangement in the interior of the *conus arteriosus* differs considerably, inasmuch as the valves are disposed in two or three transverse rows, of which, however, one only is fully developed.

We have mentioned above that the Barramunda can breathe free air as well as air dissolved in water, and we may infer this from the perfect development of the *gills* and of a *lung*. There are four gills on each side; they are broad, lamellated membranes, free from each other, but attached to the outer walls of the gill cavity, which peculiarity is clearly an approach to the fixed gills of the sharks and rays. The pneumatic apparatus may be described either as a single lung, with symmetrical arrangement of its interior, or as two lungs confluent into a single sac without a dividing longitudinal septum. Its interior is divided into about thirty compartments on each side, formed by strong transverse septa and cellular at the bottom; it is evident that by this arrangement the respiratory surface is much increased in extent. This pulmonary sac extends from one end of the abdominal cavity to the other, and terminates anteriorly by a short duct in a *glottis*, viz. a slit with a valve in the ventral side of the œsophagus, somewhat to the right of the median line. In this respect the Barramunda shows itself to be a true fish, in spite of the presence of a lung, inasmuch as it is obliged to receive the air through the mouth, whilst nearly all Batrachians have the palate perforated by the nostrils, which form a distinct passage for the air used in breathing. When the fish sojourns in pure water, and breathes by the gills, the lung does not differ from the air bladder of other fishes; it then receives arterial blood, returning venous blood, like all the other organs of the body; but when the respiratory function rests with the lung, the pulmonary vein carries purely arterial blood to the heart, where it is mixed with venous blood and distributed to the various organs.

Externally the *intestinal tract* appears as a wide straight sac without divisions or circumvolutions. Internally it is traversed throughout by a spiral valve performing nine gyrations; the cavity before the commencement of the valve must be regarded as the stomach. The liver does not show any peculiarity.

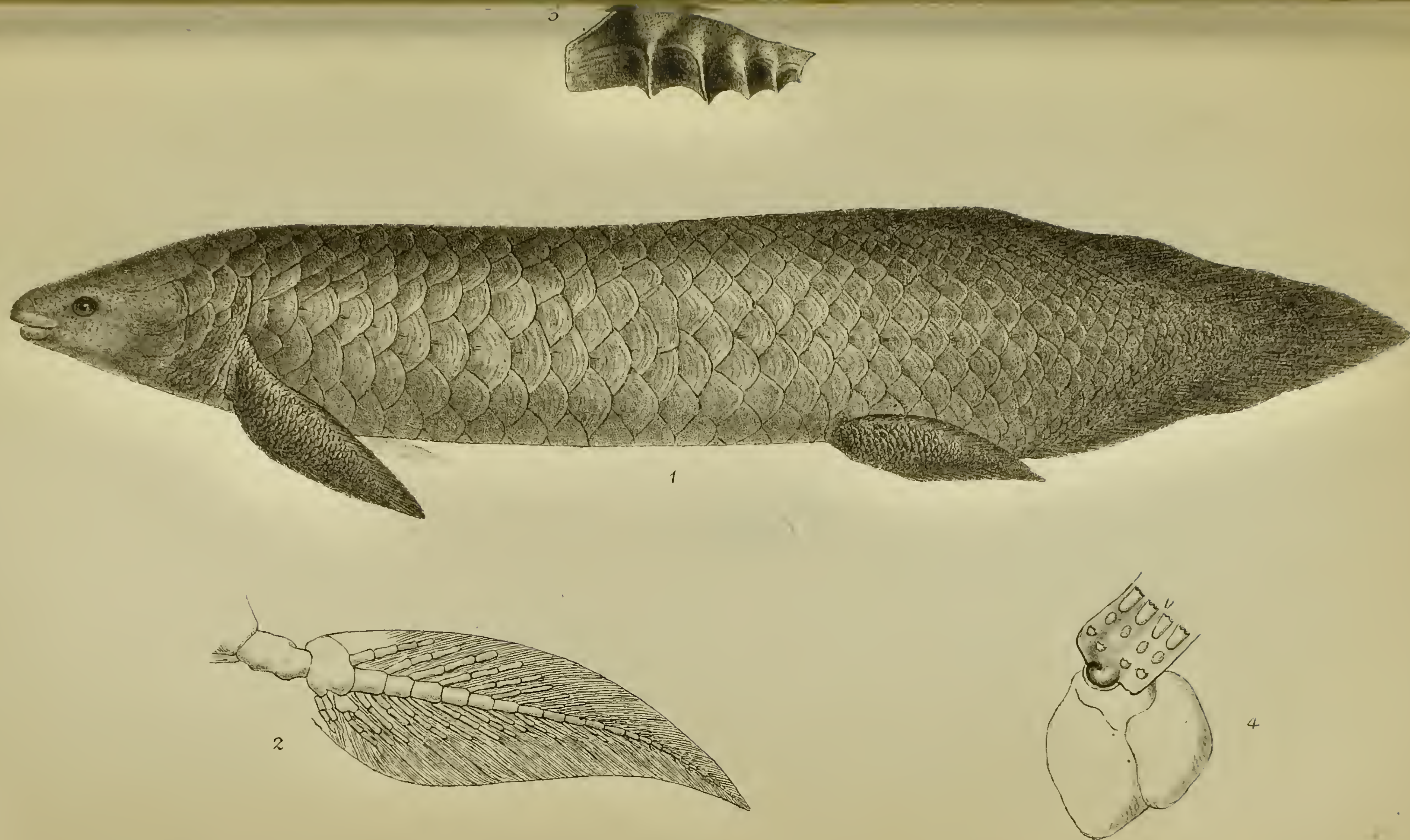
The *kidneys* are paired, the ureters entering a very small urine bladder at the back of, and partly confluent with, the rectum.

The *generative organs* are paired. Their products pass outwards by a paired oviduct or vas deferens. These ducts are entirely separate from the ovaries or testicles, each having a distinct abdominal orifice immediately below the diaphragma. They accompany the ureters in their posterior course, but are nowhere confluent with them, and terminate in a common opening into the cloacal dilatation, immediately in front of the uretral orifice. The ovaries are elongate bands, their outer surface being crossed by a great number of lamellæ, the bearers of the stroma in which the ova are developed. The ova are in very great number, and when mature drop into the cavity of the peritoneum, as in Salmonoids; but instead of being expelled at once by the peritoneal openings, they travel along a shallow gutter *forwards*, enter the much convoluted oviducts, where they receive a gelatinous covering, like the eggs of Batrachians, and are finally expelled through the cloaca. The testicles and vasa deferentia are analogous to the female organs with regard to position, form, and orifices.

Such are the principal points in the organisation of the Barramunda; and it remains now to add some remarks on its affinities and on the bearings which the acquaintance of this singular type has upon the advancement of science.

1. When we direct our enquiry at first to recent fishes, there cannot be any doubt as regards the close relationship between *Ceratodus* and *Lepidosiren*. The latter had been regarded by Joh. Müller (and by most subsequent ichthyologists) as the type of a separate sub-class—*Dipnoi*—which he distinguished from the Ganoids by the presence of a pair of longitudinal valves in the conus arteriosus of the heart, the valves being arranged in transverse series in Ganoid and Plagiostomous fishes. We see now that the valvular arrangement in *Lepidosiren* is merely a modification of the Ganoid heart, and that the characteristic feature of the latter consists in the presence of a pulsating third division—the *conus arteriosus*. Therefore we are compelled to abandon the sub-class, *Dipnoi*, and to refer it as a sub-order to the Ganoids, with its definition altered thus: The *Dipnoi* are Ganoid fishes with the nostrils within the mouth, with paddles supported by an axial skeleton, with lungs and gills, with a notochordal skeleton, and without branchiostegals.

2. But it appears to me that *Ganoids* and *Chondropterygians* (sharks and rays) are much more closely allied to each other than either of them to the sub-class of Teleosteans, which comprises the majority of the fishes of the present epoch, and the members of which have, instead of a contractile conus arteriosus, simply a non-contractile swelling of the aorta, separated



Dr. Günther

A ganoid Fish from Queensland (Ceratodus)

W. West & Co. lith.

from the heart by two valves opposite to each other. Nor is this the only distinguishing character. A heart with a true conus arteriosus is always accompanied by a more or less developed spiral valve of the intestine (entirely absent in Teleosteans), and by non-decussating optic nerves. The fore and hind limbs of the Chondropterygians are also paddles supported by a cartilaginous framework; the tooth-bearing pterygo-palatine arch of the *Dipnoi* is homologically identical with the "upper jaw" of a shark. And the anotomical evidence in favour of a union of Ganoids and Chondropterygians is rendered complete by the *Holocephala* (Chimæras), which differ in several important points from the other Chondropterygians, approaching the Ganoids by these very characters, and are, in fact, an intermediate form. They are sharks in external appearance and with regard to the structure of their organs of propagation. On the other hand, there is only one external gill opening on each side; the skeleton is notochordal, and the palatal apparatus coalesces with the skull as in *Dipnoi*, which is not the case in any of the sharks and rays; likewise the dentition approaches that of *Ceratodus*. Sir Ph. Egerton has drawn attention to another very important fact; viz. that the dorsal spine is articulated to the neural apophysis, and not merely implanted in the soft parts and immovable, as in sharks. Furthermore, all those modifications which show an approach of the ichthyic type to the Batrachians are found in Ganoids and Chondropterygians, none in Teleosteans; and, finally, the coexistence and development of Ganoids and Chondropterygians in geological epochs when no (or only very few) Teleosteans existed, is a circumstance which seems to confirm a conclusion arrived at from an anatomical point of view only; namely, the conclusion that Ganoids and Chondropterygians should be united in one sub-class—*Palæichthyes*.

3. A third point of the deep interest is the great antiquity of the Dipnoous type. At the commencement of these notes we have seen that there is no evidence to show that the Barramunda is even generically distinct from those fishes, of which, unfortunately, the teeth only have been preserved. But some of the oldest fishes, known from the Devonian epoch, and designated by the names *Ctenodus* and *Dipterus*, prove to be Dipnoous fishes. They had the same dentition as *Ceratodus*, nostrils within the mouth, acutely lobed paddles, a notochordal skeleton, and, with exception of dermal scutes, a very similarly formed skull. Thus, then, we have the following facts before us: The Dipnoous type is represented in the Devonian and carboniferous epochs by several genera (*Dipterus*, *Ctenodus*, *Chirodus*, *Conchodus*, *Phaneropleuron*); it is then lost down to the Trias and Lias, where the scanty remains of a distinct

genus (*Ceratodus*) testify to its presence; no further trace of it has been found until the present period, where it reappears in three genera (*Ceratodus*, *Lepidosiren*, *Protopterus*), one of which is identical with that of the Mesozoic era. Now, at present scarcely any zoologist will deny that there must have been a continuity of the Dipnoous type, and it is only a proof of the incompleteness of the palæontological record that we have to derive all our information regarding it from only three so very distant periods of its existence.

In conclusion I may add a synoptical table, from which the systematic views advocated above, and more especially the position of *Ceratodus* in the system, may be readily understood. After the separation of *Amphiosus* and the Lampreys as types of two distinct sub-classes (*Leptocardii* and *Cyclostomata*), the remaining host of fishes are referred to two other sub-classes:—

SUB-CLASS: *Teleostei.* Heart with a rigid bulbus aortæ; intestine without spiral valve; optic nerves decussating (living species, nearly 9,000).

SUB-CLASS: *Palæichthyes.* Heart with a contractile conus arteriosus; intestine with a spiral valve; optic nerves non-decussating.

- *Order I.—Plagiostomata*, or *Marine Palæichthyes* (sharks and rays; living species, nearly 300).
- *Order II.—Holocephala* (four living species).
- *Order III.—Ganoidei*, or *Freshwater Palæichthyes.*
    - Sub-order 1.—*Amioidei* (one species).
    - Sub-order 2.—*Lepidosteoidei* (three species).
    - Sub-order 3.—*Polypteroidei* (two species).
    - Sub-order 4.—*Chondrostei* (sturgeons, thirty species).
    - Sub-order 5.—*Dipnoi.*
        - Fam. a.—*Sirenidæ.*
            - Sub-fam.—*Ceratodontina* (Ceratodus).
            - Sub-fam.—*Protopterina* (Lepidosiren, Protopterus).
        - Fam. b.—*Ctenododipteridæ* (Ctenodus, Dipterus).
        - Fam. c.—*Phaneropleuridæ* (Phaneropleuron).

GREENWICH OBSERVATORY.

By JAMES CARPENTER, F.R.A.S.

THERE are few scientific institutions whose objects are so little understood, and whose labours are therefore so likely to be misjudged, as an astronomical observatory of the character of the national one at Greenwich. Even those who possess some knowledge of astronomy, who read its literature, and take a warm interest in its salient achievements, are frequently little or not at all conversant with those departments of the science that are perforce pursued in an essentially practical establishment, where the sun may be observed day by day without a moment's thought being given to his spots, the moon watched by night without a care for her physiography, and where the planets and stars are subjects of a system of close observation which, however, gives no heed to questions concerning their physical nature.

It happens, from a circumstance that will bye-and-bye appear, that the present is an opportune time for reviewing the history of the Observatory at Greenwich and its relation to current astronomical science; but it may be mentioned that the appearance of this article at this opportune time is merely accidental.

To start with a just idea of the very definite aims of the Observatory, we should clearly recognise the circumstances that led to its foundation. It was born of a necessity that arose from that extension of British navigation which was, at least partially, a consequence of the passing of the Navigation Act of Charles II. The necessity was a means of obtaining the longitude at sea. The latitude, we may remark, presented no difficulty whatever. A method for longitude had for more than a century existed in theory; for Apian in 1524, Gemma Frisius a few years later, and Kepler subsequently, had proposed the use of lunar distances in the very form that now universally obtains. The method may be thus described. The moon moves rapidly among the stars. Suppose that for a given instant of Greenwich

time her angular distance from a near star be known beforehand. Then if an observer at sea measure the distance minute by minute till he makes an observation which shows the distance * equal to the given one, he knows the Greenwich time for the moment of that observation; and this time compared with the local or ship's time of the observation gives, by mere difference, his longitude reckoned, in time, from Greenwich. If the distance of moon from stars be given at fixed intervals of Greenwich time, say hourly,† then it matters not when the navigator takes his distance, for he can always find, by interpolation, the instant of Greenwich time corresponding to the moment of his observation.

Now, to make this method practical two things are necessary. First, the positions of the fixed stars must be exactly known; and second, the moon's place hour by hour must be accurately predictable a long time in advance, so that the mariner may carry the table of predicted Greenwich distances out with him. The plan, as we have said, was proposed more than three centuries ago, and the need of applying it was severely felt just two centuries ago. But there were no catalogues of fixed-star plans accurate enough for the purpose; and the knowledge of the moon's motions was utterly insufficient also. The double want was brought to the King's (Charles II.'s) notice, and he at once ordered an observer to be appointed and an observatory to be founded to meet it. The right man for the post turned up in the person of Flamsteed, and he was commanded forthwith to "apply himself with the utmost care and diligence to the rectifying the tables of the motions of the heavens and the places of the fixed stars, so as to find out the so-much-desired longitude of places for the perfecting the art of navigation." The same object is set forth in the tablet which still stands over what was the entrance-door of the building; and a wording almost identical with that just quoted has been maintained in the warrants of all succeeding Astronomers Royal to the present day.

It is desirable that this definition and its implied limitation of the duties of the Observatory should be borne in mind. There is no provision in its charter for the numerous modern subjects of inquiry which have been classed under the head of *astronomical physics*. It is true such subjects have been occasionally followed up, but only to an extent that precluded

* Certain corrections are necessary to reduce the angular distance as measured at the earth's surface to what it would be if measured from the earth's centre; but these form part of the after calculation.

† As a matter of fact, they are given for every third hour in our "Nautical Almanacs."

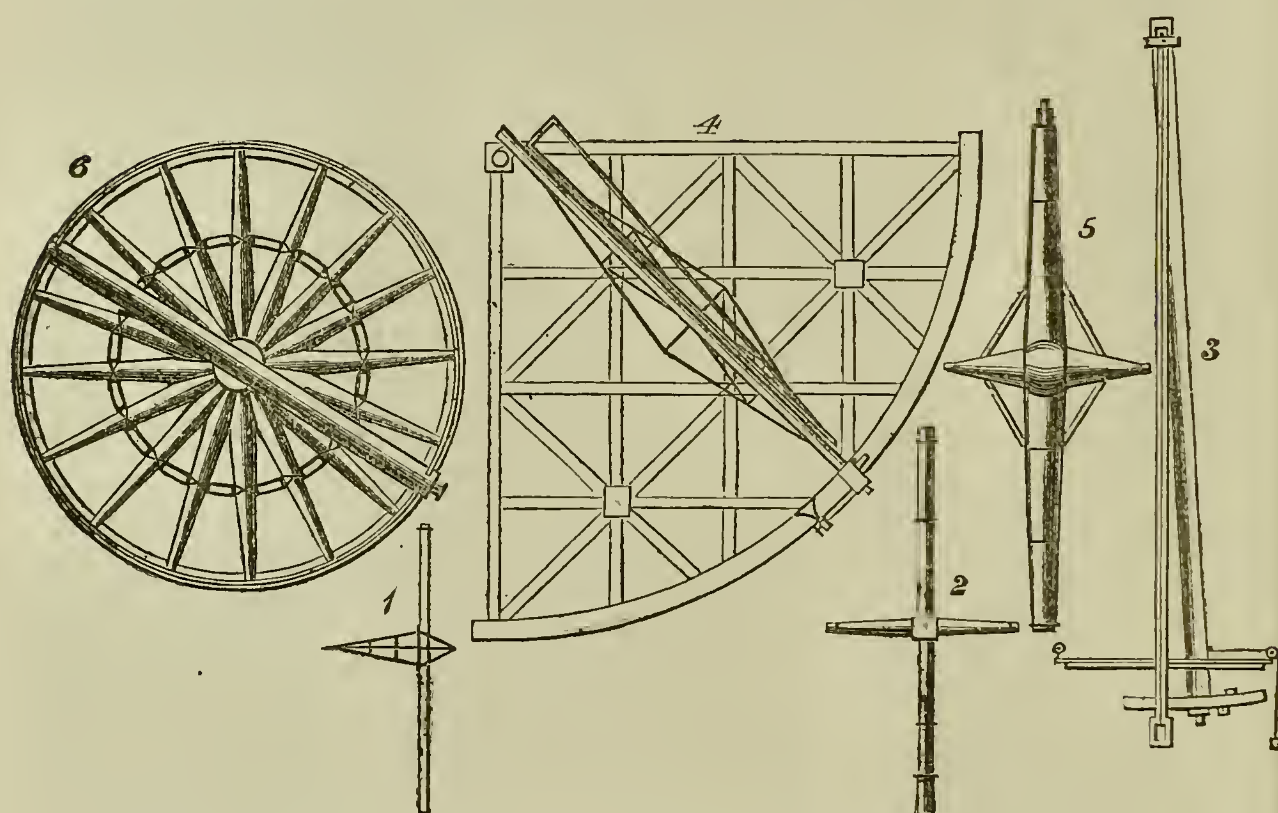
interference with the primary objects of the institution. From first to last Greenwich has been held to be an institution for the pursuit of mensurative astronomy with utilitarian ends, and its instruments, and, to some extent, its *personnel*, have been provided and organised accordingly. The succession of Royal Astronomers—Royal Observators, as they are officially styled—Flamsteed, Halley, Bradley, Maskelyne, Pond, and the present occupant of the office, have, with one exception, laboured with a consistency truly remarkable, like one long-lived man, to carry out, with the best resources of their times, the strictly fundamental works committed to them: with what results we shall presently see.

Positions of stars for the formation of catalogues of star-places, and positions of the sun, moon, and planets for the ultimate formation of tables of their motions, are the staple observations at Greenwich, and they consist almost entirely of meridian observations, namely, times of transit over the meridian, for determination of objects' Right Ascensions, and measures of the angular distances of the same objects from the celestial pole, for the determination of their North Polar distance; these last being also made on the meridian. The instruments for these observations are the "Transit" and some form of "Circle," both instruments moving in the plane of the meridian only, and the first having it accurately defined by a vertical wire, in modern times a cobweb thread. Flamsteed at first had a make-shift sextant, with which he could only measure one star from another in a straight line, and he had to leave these measures to be ultimately referred to fixed points for determination of Right Ascensions and Polar Distances. But in 1689, thirteen years after his appointment, he procured—from his own resources, by the way, for from first to last he never had a penny for instruments from the Government—a large graduated arc, which was fixed upon a wall in the meridian plane, and upon the centre of which was pivoted a telescope with a vertical wire at its focus; and he took clock-times of transit across this wire for Right Ascension, and read the position of the telescope upon the arc for Polar Distance. Halley had a transit instrument, with a $1\frac{3}{4}$-inch object-glass, generally similar in plan to those of the present day, specially for R.A. observations, and a meridianal quadrant of 8 feet radius specially for those in Polar Distance. Bradley at first had Halley's instruments, afterwards a better transit of $2\frac{3}{4}$-inch aperture, and a second quadrant, so that he could command the whole meridian from the North horizon to the South. Maskelyne used Bradley's instruments from first to last, but the defects of a quadrant, in its liabilities to distortion and errors of centering, were so obvious to him, that just before his

death he ordered a complete circle, of 6 feet diameter, to be made, with a telescope, of 4-inch aperture, fixed upon it, graduated around its periphery. This circle turned upon a pivot carried through a wall to which were fixed six micrometers for reading the circle divisions. Maskelyne did not live to use this "Mural circle," but Pond had the advantage of it; and he subsequently had another one made like it, using the two together. Pond also had a new Transit made by Troughton, of 5-inch aperture, which was a masterpiece; and with these means he pushed observing accuracy to a point that had not before been aimed at, and has even now scarcely been surpassed.

The essential parts of all these instruments (except Flamsteed's) are shown in the accompanying sketch, upon a scale of

Fig. 1.



ANCIENT GREENWICH INSTRUMENTS.*

1. Halley's Transit (1721–1749).
2. Bradley's Transit (1750–1816).
3. Bradley's Zenith Sector.
4. Mural Quadrant (1725–1812).
5. Troughton's Transit (1816–1850.)
6. Mural Circle (1812–1850).

about $\frac{1}{4}$ inch to the foot: the cut also shows the form of Bradley's famous Zenith Sector, with which he consummated his immortal aberration discovery.

The present Astronomer Royal, Sir George Airy, used Pond's instruments as he found them until about the year 1850, when finding them too small in their object-glasses for existing and future wants—especially for the observations of the newly-dis-

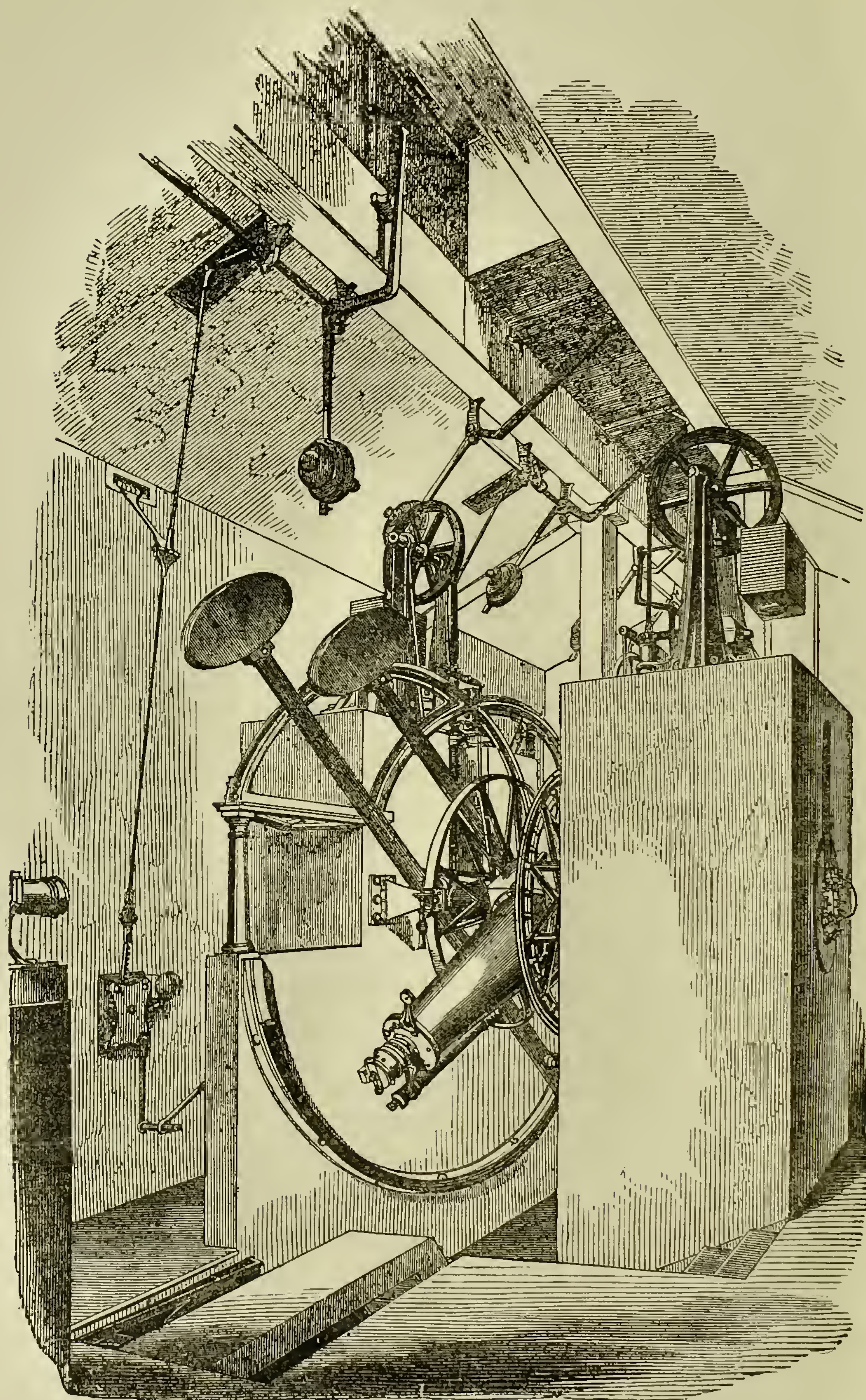
* The woodcuts illustrating this article have been kindly lent by Messrs. Bell and Daldy, and Messrs. Bradbury and Evans.

covered minor planets—he made a revolutionary change of double character, first, by designing an instrument which, while carrying a large object-glass, should in itself combine the functions of both a Transit and a Mural Circle, and second, by causing such an instrument to be constructed of a solidity hitherto undreamt of. The engineer became instrument-maker in place of the optician, and cast and wrought iron-work of the former supplanted the tender and perishable brass of the latter. Not that the optician was ignored, but his work was confined to the delicate parts, the glasses, micrometers, and divided circles; and he and the engineer worked upon this instrument, as in all later additions to Greenwich, in judicious concert under one head.

The Transit Circle consists essentially of a telescope, 12 feet long with an object-glass of 8 inches diameter, turning between two massive stone piers. The telescope tube and the pivots are of cast iron strengthened by internal braces. In the focus is the series of vertical cobweb wires, the central one marking the meridian, for observations in Right Ascension. Upon the western side of the axis (right hand in the cut) is a circle 6 feet in diameter, divided on silver to five minutes of arc, and read by six long micrometers whose eye-ends are on the other side of the pier, which is pierced for the purpose. The micrometers read to ·01 of a minute. The circle reads Zenith Distances, which are afterwards converted into North Polar Distances. One horizontal wire in the telescope, whose position is registered by a micrometer screw on the eye-piece, is used to bisect the star or object under observation, and the reading of its micrometer is combined with the circle reading. Upon the eastern axis (left hand in the cut) is another circle; this is for clamping only. A trough of mercury (for obtaining the horizontal point of the circle by observation of the same star directly and by reflexion) is carried on a circular tramway on the eastern pier by means of parallel bars which, with the counter-weights of the trough, appear in the picture: the trough does not appear. North and south of the instrument are two fixed inverted telescopes (the end of one appears in the cut); these are for giving a line of collimation: each has a wire at its focus which can be viewed by the other through an aperture in the central cube of the great telescope, or by raising the great telescope from its bearings, for which there is due provision; the wires are adjusted to coincidence, and then by observing each with the great telescope the collimation error of the latter is obtained. The instrument is never reversed. The error of level of the axis is found by measuring the amount of non-coincidence of the meridional cobweb with its own image reflected from a trough of mercury placed perpendicularly under the telescope.

In front of the observer, as he looks south, is the *Transit clock* (not seen in the picture) for observing times of passage

Fig. 2.



THE TRANSIT CIRCLE.

of objects over the vertical wires. Formerly these times were taken by ear, the observer counting the clock-beats and noting

the second and decimal of a second at which the object passed each wire. Since 1851 they have been registered by an electric chronograph. A sheet of paper on a cylinder rotating uniformly receives a puncture from a point on the armature of an electro-magnet at every beat of the Transit-clock: thus a time scale is formed by the clock. The observer taps a key mounted on the telescope eye-piece as the object under observation crosses each wire; his tap completes a galvanic circuit which works another electro-magnetic pricker; his punctures fall among the clock punctures, and their times can be read off thereby. The prickers travel down the cylinder as it rotates, and thus give a spiral line of registers. Uniform rotation of the cylinder being of the highest importance, it is secured by a driving-clock controlled by a *rotating* in place of a vibrating pendulum.

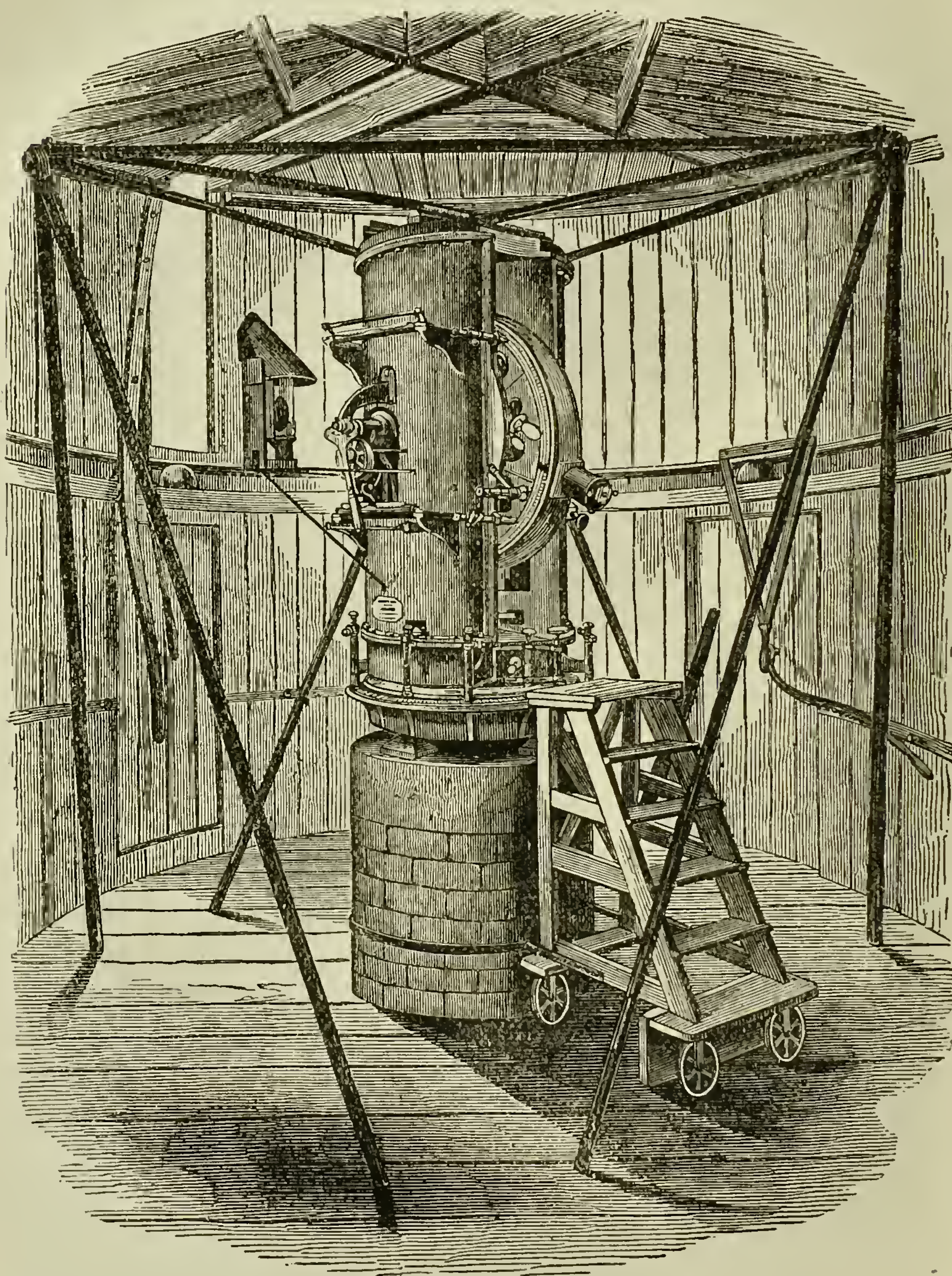
Since the *Transit-clock* is a measuring instrument of the most accurate kind, its excellence and the steadiness of its rate should be of the highest character attainable. The one in the Transit Circle room at Greenwich having grown old and exhibited some slight infirmities, a new Sidereal Motor has lately been mounted, in a basement where the temperature is nearly uniform, at a distance from the Transit instrument. It is, however, in connection with the chronograph, and nearly all Transit observations are made (galvanically) by it. It also gives controlling currents to several subsidiary clocks. Its rate is wonderfully regular; so uniform, indeed, that it faithfully exhibits the small changes which are produced by barometric fluctuations.

The Transit Circle being the chief instrument of the Observatory, it is in almost constant use whenever the sky is clear. A daily course of work with it includes Transits and North Polar Distances of the sun, moon (when visible on the meridian), all the large planets that pass before 3 o'clock in the morning, all the small planets passing between 10 P.M. and 1 A.M. during the first half of the lunation,* transits of a selection of the principal or fundamental stars for error and rate of the clock, an upper and lower culmination of a polar star for azimuthal error, an observation of the reflected meridional wire for level error, and of each collimating mark for collimation error of the Transit telescope; and observations of four stars, two north, two south of the zenith, each by reflexion and directly, for the horizontal-point error of the circle. These secured, as many extra or small stars as can be are observed for the ultimate formation

* By arrangement with the Paris Observatory the small planet observations are divided; Greenwich observing from new to full moon, and Paris from full to new moon.

of catalogues, or for special purposes. Certain instrumental errors, such as flexure of the telescope, errors of circle divisions, and want of circularity of pivots, are determined occasionally. Nothing is supposed for a moment to be in perfect adjustment;

Fig. 3.



THE ALTAZIMUTH.

no error is *mechanically* corrected; everything is supposed to be always wrong, and to be corrected *numerically* in the after reduction of the observations.

But the Transit Circle has an important auxiliary in the Altitude and Azimuth, or, as it is briefly called, the *Altazimuth* instrument, which was designed specially for position observations of the moon. Viewing the primary importance of these

observations, and having regard to the impossibility of seeing the moon on the meridian during that critical part of her orbit when she is near the sun, and also to the frequent loss of meridional observations from temporary cloudiness of the sky, Sir George Airy deemed it highly desirable to devise means of securing observations *off the meridian* comparable in accuracy with those made on the meridian. The only form of instrument competent for this purpose was an altazimuth of massive construction. He accordingly designed and mounted (in 1847) the instrument here figured. It consists of a telescope (4 inches aperture) solidly fixed into a broad-rimmed vertical or altitude circle which has a divided silver limb read by four microscopes. This circle turns between two semi-cylinders of cast-iron connected by top and bottom plates to form a frame which turns on vertical pivots, the lower one borne by a stone pier and the upper one by a triangular framework of iron bars. The vertical frame carries on one side the four microscopes which read the altitude circle, and around its base other four which read a horizontal circle fixed to the stone pier for azimuthal measures. There are levels for showing the inclinations of both axes; and they, and the microscopes, are carried by supports which are cast upon (not screwed to) the parts they spring from. The nightly observations with this instrument consist of an altitude and azimuth of the moon, azimuths of stars for zero of the azimuth circle and collimation error of the telescope, and observations of a distant terrestrial mark for zenith-point error of the vertical circle. All observations are made in two (reversed) positions of the telescope; and all times of exact position-determinations are registered by the chronograph. The azimuths and altitudes are in effect reduced to Right Ascension and Polar Distances by after-calculations, which are very laborious, but are, in common with every computation in the observatory, facilitated by skeleton forms of which about two hundred, simple and elaborate, are in constant use.

The Altazimuth secures about two hundred observations of the moon in a year, the Transit-Circle only one hundred; and a large percentage of the former are of the highest value as affording tests of the lunar tables for parts of the orbit when the moon is near conjunction with the sun, and cannot be touched by a meridian instrument.

Another auxiliary instrument of the exact class is the Reflex Zenith Telescope, for measuring with great precision the small zenith distance of the star γ Draconis, which passes near the zenith of Greenwich, and is favourably situated for determination of the amount of stellar aberration. It consists of a horizontal object-glass, capable of semi-rotation about a vertical axis, with a trough of mercury at half its focal distance below.

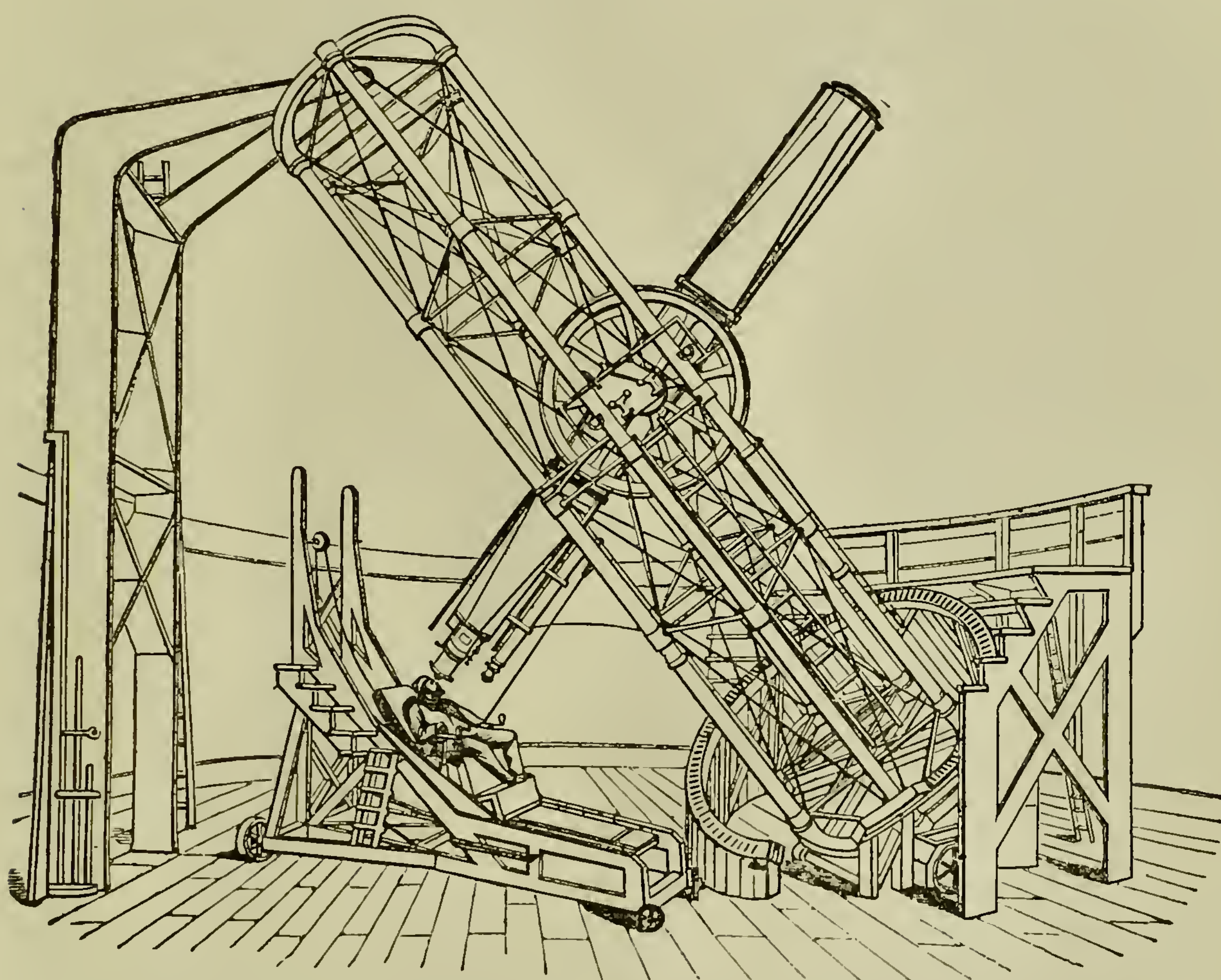
The rays from the star, after passing through the object-glass, are received upon the mercury and reflected up through the glass again, forming a focal image just above it. In the focal plane are the wires of a micrometer by which the star's image is twice bisected, the object-glass being reversed between the bisections. A measure of the star's zenith distance is thus obtained which is remarkably free from instrumental errors. During the past two years a zenithal telescope of 3-feet focus, filled with water, has been used upon the same star to determine whether the aberration is altered in amount by the passage of the star's light through a considerable thickness of a refracting medium—a question which has been raised by some continental astronomers. The observations hitherto have negatived the anticipated alteration.

Equatorially-mounted telescopes have always been regarded as of secondary importance at Greenwich. When Sir George Airy entered office he found the equipment in this department, to use his own word, "contemptible." The best equatorial then was one presented by Sir G. Shuckburgh, made by Ramsden, with a 4-inch object-glass, a shaky mounting, and in a very bad situation. In 1837 a $6\frac{3}{4}$-inch glass was presented to the Observatory by the late Rev. R. Sheepshanks; it was a somewhat inferior one, and it was mounted with circles too small for accurate measurements. So about fifteen years ago Sir George Airy planned the Great Equatorial, deeming it essential that the Observatory should have a first-rate instrument of the class, even if it was for a time regarded as a luxury. The instrument (without the covering dome) is figured in the annexed cut. Its object-glass, by Merz, of Munich, is of $12\frac{3}{4}$-inch aperture, and 17 feet 6 inches focus. The mounting is of the English form, which admits of large circles, and such were regarded as essential; the hour circle is 6 feet diameter, that for Declination 4 feet. The polar frame is formed of iron columns firmly braced, and connected at top and bottom by oval iron frames. Except the tube, which is of wood, every large part is of iron, and the whole was designed with the highest engineering skill. Gas is carried to all the micrometers about the instrument, and to the eye-piece; and galvanic wires are led to a chronographic touch-piece at the eye-end and to a chronometer there also, which is always in front of the observer at the telescope, and is controlled by the Sidereal Motor Clock. An observing chair, moving up and down upon a travelling frame, enables the observer to command all positions without leaving his seat. The driving-clock is mounted in a lower apartment; it consists of a water turbine controlled by a conical pendulum.

We have spoken hitherto solely of the tools of the Greenwich

observers. Let us now pass in review the works that have been actually accomplished thereby in furtherance of the objects for which the Observatory was constituted. We begin with the formation of star catalogues, for these are the foundations of exact astronomy. Flamsteed formed a catalogue of 3,310 stars, that remained the standard work of its kind for half a century, and has, in respect of selection and nomenclature of the stars it comprised, served as the basis of every catalogue since produced. Halley did nothing in this department. Bradley worked wonders; he laid the foundations of the present fabric

FIG. 4.



THE GREAT EQUATORIAL.

of mensurative astronomy. In 1750, when his new instruments were mounted, he began the daily course of meridional observations of principal stars, sun, moon, and planets which has continued without interruption (save by bad weather) to the present time. His star observations were incorporated into a catalogue (which comprised 3,222 stars) by Bessel, and, with values of the constants of refraction, aberration, precession, and other elements deduced therefrom, was brought forth by the illustrious Königsberg astronomer in a great work, whose title expresses its character—we allude to the "Fundamenta Astronomiæ." The rich harvest that Bradley reaped has not yet

had all its good grain thrashed out, for at this moment several German astronomers are engaged in a re-reduction of his observations, believing that they can get from them even more than Bessel obtained. Maskelyne did not aim at producing a great catalogue; he confined himself to thirty-six of the principal stars, whose places he sought to fix with the utmost possible exactness, in order that they might serve as reference-points of the first order. Pond accumulated a long series of observations, and produced a catalogue of 1,112 stars, which was the most valuable contribution to the sidereal astronomy of the time, and is second in accuracy to no modern catalogue. The present Astronomer Royal has already produced four large catalogues; the first from the observations during the twelve years 1836–47; the second from the six years 1848–53; the third from the seven years 1854–60, and the fourth from the seven years 1861–67. The numbers of stars in these catalogues average over 2,000 each; each contains all the fundamentals, and together they include the greater portion if not all the stars visible at Greenwich down to the fifth magnitude, and the majority of the sixth. They are sought all the world over, wherever accurate astronomy is pursued for its own sake or for geographical or geodetical purposes, and they have served and will serve again as the best materials for researches upon the proper motion of the stars or of the solar system.

Then as to the moon; Greenwich has alone sufficiently supplied the investigators of her movements from the time of Newton to the present. At so critical a period was the Observatory established that Flamsteed actually fed Newton, so to speak, from hand to mouth with places of the moon for perfecting the lunar theory which the latter was then deducing from his theory of universal gravitation; and there is an entry in Flamsteed's note-book of the author of the "Principia" coming down to Greenwich one Sunday evening for twelve observations of the moon of which he was in urgent need. Halley, though he did no star work, made some moon observations, and compared them with his own lunar tables; it was the only good piece of regular work that he did. He was the exception we previously spoke of to the consentaneous character of the Royal Astronomers; and his case shows that a man may be a great astronomer, and yet fail in the office of Royal Observator, which requires the exercise of no small amount of self-denial. The lunar observations of Bradley, Maskelyne, and Pond, have to be spoken of connectedly, for they were reduced *en masse* by the present Astronomer Royal, and they form the basis of the Lunar Tables that are now in almost universal use. But before this stupendous uniform reduction, the Greenwich Observatory had furnished 1,200 moon observations to improve

the tables of Mayer, which were the first generally available for nautical longitudes; 3,000 were employed by Burg for his tables, which so completely satisfied the conditions of a prize for such tables offered by the Consular Government of France, that the First Consul doubled the prize ; and of the 4,000 which were employed by Burckhardt to correct the lunar elements for his famous tables (which served for nearly fifty years prior to 1862 as the basis for all navigational predictions), nearly the whole must have been derived from Greenwich.

The great lunar reductions previously alluded to embraced nearly 9,000 Greenwich observations of the moon, made between the years 1750 and 1830—a series without a parallel. The first fruit of their reduction was a general correction by Sir George Airy of the received elements of the moon's orbit. The next was the discovery by Prof. Hansen, of Gotha, of two inequalities of long period in the moon's motion, depending upon the direct and indirect action of the planet Venus. And what may be considered for the present as the ultimate outcome, was the construction by Hansen of the great Lunar Tables that bear his name, which represent the motions of our satellite with an accuracy surpassing all others, and abundantly sufficient for the preparation of reliable nautical ephemerides. Hansen's tables are used for all the important "Nautical Almanacs" of the world, with one exception, that of America, for which special tables were previously prepared, embodying, however, the corrections derived from the long suite of Greenwich observations.

If we look to other planetary tables, we find the same dependence for their data upon our National Observatory. The tables now used for Jupiter, Saturn and Uranus are those by Bouvard (1821), and they depend mainly upon the observations of those planets by Bradley, Maskelyne and Pond. The tables of Mercury, Venus, and Mars are Le Verrier's, based chiefly upon the Greenwich observations from 1750 to 1830, which were, with the rest of the planetary observations for that period, reduced by Sir George Airy *en masse*, like the lunar observations. For the sun, Le Verrier's Tables are also used ; and they depend upon a century's Greenwich observations. The current tables of Neptune are those by Professor Newcomb, for which Greenwich found the major part of the observational data. Throughout the whole series of Planetary Tables that belong to the period of accurate astronomy, there is such a broad reliance for data upon Greenwich, and such comparatively small support derived from other places, that it is evident there was no excessive flattery in Baron Zach's assertion that our astronomical tables would have been as perfect as they are if no other observatory had ever existed. All this we adduce, not with the idea of glorifying a

national institution, but merely to aid the full conception of its mission, which was defined in Sir John Herschel's always happy words to be "to furnish now and in all future time the best and most perfect data by which the laws of the lunar and planetary movements as developed by theory can be, compared with observations."

It will be easily seen that with all things organised to this end there is little room for such work as double-star measures, celestial photography, delineations of planets and nebulæ, spectroscopy, &c. Some of these have occasionally been taken up for a time, but none of them have been, or indeed could be, systematically followed.

Of late, however, there has been a tendency in some subjects of this character to overrun the powers of attention of amateurs, to whom they have been left; and it has been suggested that inasmuch as they ought to be followed by the State, and Greenwich as at present constituted could not undertake the work, a special Observatory ought to be established and devoted to Astronomical Physics. The systematic record of solar phenomena (sun-spots, gaseous eruptions, &c.) has been mentioned as in immediate need of pursuit. Not unnaturally a counter question arose whether all that it was desirable for the State to undertake could not be done at Greenwich, and the Astronomer Royal laid his views upon the general question before the Board of Visitors at their meeting on June 1 last, in the following terms:—

"The tendency of late discoveries and consequent discussions in astronomy has been, not to withdraw attention from the exact departments of astronomy, but to add greatly to the public interest in those which are less severely definite. And this has become so strong, that I think it may well be a subject of consideration by the Board of Visitors whether observations bearing upon some of those trains of discovery should not be included in the ordinary system of the Royal Observatory. The criteria which, as appears to me, may be properly adopted in the selection or rejection of subjects of observation are these: Observations which can be made at any convenient times, which do not require telescopes of the largest size, and which do not imply constant expense, ought to be left to private observers. Observations which demand larger telescopes, and especially observations which must be carried on in continual routine and with considerable expense, can only be maintained at a public observatory. The claims of each subject must be separately considered; but there can be no doubt that a very powerful demand for attention is made when private persons have been induced to continue observations for a long time at considerable current expense, and when plausible

evidence is given of the connexion of results thus obtained with other cosmical elements. I think that these considerations exclude measures of double stars at the Royal Observatory, but they leave an opening for the scrutiny of nebulæ, planets, &c., and possibly (but I speak in doubt) of solar spectroscopy. But I have no doubt that they fully sanction the undertaking a continued series of observations of solar spots. The character of the Observatory would be somewhat changed by this innovation, but not, as I imagine, in a direction to which any objection can be made. It would become, *pro tanto*, a physical observatory; and possibly in time its operations might be extended still further in a physical direction."

Upon the effect of these statements it would be premature to speak. It is, however, generally understood that the Board decided upon the advisability of extending the Observatory system so far (for the present) as to include regular photographic record of solar spots and systematic solar spectroscopy. The Visitors are an intermediary body: before full effect can be given to their decision the Treasury must be appealed to for funds for the first cost of instruments and the running expense of an increased personal staff.

Chronometers have such a direct connection with navigation and sea-longitude, that not unnaturally Greenwich has been identified with the testing of these instruments from their invention to the present day; and now it is the chief depôt for Government marine chronometers. All business of their purchase, trial, and repair, is transacted at the Observatory; and usually about 200 chronometers are there under rating for issue to H.M. ships. Every year there is a competitive trial open to all makers, during which the chronometers are exposed to a wide range of temperature: four or six of the best instruments are each year purchased at good prices, and it is doubtless to these trials and to the general Government patronage of the trade that the supreme excellence of British chronometers is due.

Into the department of Time and its distribution we need not enter further than to say that a signal ball is dropped at one o'clock daily at the Observatory, and another ball at Deal is dropped by direct current from the Observatory; and that every hour accurate electric signals go forth from Greenwich, which are variously distributed over the country; one of them, that at 10 A.M., passing through well-nigh all the important telegraph lines in England. This department of the Observatory was, however, fully described in a previous number of the POPULAR SCIENCE REVIEW (October 1870).

A magnetical and meteorolgical department was established in 1840, and till 1847 eye-observations of its instruments

were made every two hours, day and night. In 1848 photographic registration was introduced, and from then till now there has been an unceasing record of the movements of the declination, horizontal force, and vertical force magnetometers, as well as of the barometer and the dry and wet bulb thermometers. The anemometers, for direction, force and velocity of the wind, and also a pluviometer, register themselves mechanically. The magnetic observations and registers to 1863 have been discussed, and the epitomised results form the subjects of various memoirs in recent volumes of the *Philosophical Transactions*. It may be mentioned that among other points these discussions negative the existence of a decennial magnetic period related to the period of solar spot activity. A great discussion of temperature records from 1848 to 1868 is now in progress. Within the past few years an important, and we believe unique, addition has been made to the photographic recording department. The spontaneous galvanic "earth-currents" that at times become so intense as to interfere with telegraphic operations, have been made to record themselves perpetually by reflecting galvanometers connected with special wires running in N.-S. and E.-W. directions through the Observatory, and attached to earth-plates at their extremities. A discussion of some of the registers has shown that these currents are related to the earth's magnetism in its disturbed state —as during auroral displays—but apparently not in its tranquil state.

In conclusion, it should be stated that the Greenwich observations of all kinds are published in yearly 4to. volumes of nearly 1,000 pages each, in which every observation is set down in the utmost detail, with every instrumental reading as it is recorded by the observer, and (especially in the case of the astronomical observations) with every step in the reductions exhibited, down to the final results, which are given in such a form as to be directly available to the theoretical investigator.

THE NEWLY-FOUND FOSSIL MAN

/6.

THE RECENT FOSSIL MAN.

By J. MORRIS, F.G.S.
Professor of Geology in University College, London.

[PLATE LXXXVII.]

THE subject of the antiquity of man has of late years attracted considerable attention, and the terms palæolithic and neolithic have become nearly as familiar as those of the stone and iron age of former years. For preconceived opinions on this point, and the apparent doubtful evidence of the association of the human species with those of the extinct mammalia, strengthened the belief of the appearance of man only after the great physical changes had brought about their disappearance. Hence arose, partly from want of careful observation, much controversy on the subject, and, although maintained by some, the opinion has been confirmed by the re-examination of several of the cases cited, as well as by recent discoveries, that the remains of man lie entombed in earlier graves than those where

The rude forefathers of the hamlet sleep.

Among those who carefully investigated and prominently brought the subject forward were Mr. Prestwich * and the late Dr. Falconer, after their examination of the discoveries near Abbeville by M. Boucher de Perthes and of the Brixham cave; the facts of the contemporaneity of the works of man with the remains of extinct mammals were still contested by Elie de Beaumont and others; but the opinions of Mr. Prestwich were corroborated by Mr. Evans, Mr. Flower, Sir J. Lubbock, Sir C. Lyell, and by MM. Lartet and Christy, and Gaudry in France.

The occurrence of human remains are far more rare in caverns or other deposits than those of the works of man, and hence every additional fact is worthy of careful examination.

* "Phil. Trans." 1860, Pt. II. p. 277.

Dr. Buckland, in 1824,* discovered in the Paviland Cave, Glamorgan, beneath a shallow covering of earth, nearly the entire left side of a human female skeleton, which, he states, was clearly not coeval with the antediluvian bones of the extinct species of elephant, rhinoceros, hyæna, bear, &c. found there; but that this exposed and solitary cave had, at some time or other, been the scene of human habitation, not only from the charcoal and fragments of recent bone, but that the ivory rods and rings in contact with the skeleton were certainly made from part of the antediluvian tusks that lay in the same cave. Besides the skeleton of the "Red Lady of Paviland," human bones have been fouud by Col. Wood in the "Spritsail-Tor" cave, and in the ossiferous fissure of Mewslade, in the Peninsula of Gower.

Dr. Schmerling, however, in 1833, announced the discovery, in the Engi and Engihoul Caves in the Valley of the Meuse, of the bones of man, associated with those of recent and extinct mammals—rhinoceros, horse, elephant, bear, hyæna; the bones were indiscriminately mixed together, and the cave earth did not seem to have been subsequently disturbed.

M. Marcel de Serres, in his exhaustive essay for that period (1838), on "Bone Caverns," cites many instances of the co-occurrence of human remains: in America (Kentucky) with the megalonyx, bear, deer, and bison; in Franconia with extinct species; in France, in the departments of Lozère, Gard, and l'Aude.

M. E. Lartet, in his paper on "The Co-existence of Man and the Great Fossil Mammalia," published in the "Annales des Sciences Naturelles" (Ser. IV. vol. xv. p. 178), after his examination of the Aurignac Cave, near St. Gaudens, states that, not only was man cotemporary with the mammoth and rhinoceros, but, like the natives of Africa at present, used the latter as an article of food; and that the human remains are of great antiquity, as they were associated with the bison, reindeer, megaceros, hyæna, and *Ursus spelæus*, the latter, according to M. Lartet, being the earliest which disappeared of the group of the great mammalia, and which age—the earliest of primitive man—was followed successively by the age of the elephant and the rhinoceros, the age of the reindeer, and the age of the aurochs.

The cavern of Bize and others, in the vicinity of Narbonne, are equally remarkable for the similar association of man with the *Ursus spelæus*, *Hyæna spelæa*, *Rhinoceros tichorhinus*, and reindeer, with about sixteen other species, of which the bones of the ox, deer, and horse were the most numerous,

* "Reliquiæ Diluvianæ," p. 87.

together with the remains of birds. These were first noticed by M. Tournal in 1827, and subsequently by MM. M. de Serres, Christol, Dumas, P. Gervais, and Brinckmann, the latter two authors giving an account of their observations in the " Messager du Midi " (Montpellier, 1864), and they remark that the occurrence of the reindeer does not indicate so great geological antiquity, although still remote, but that the climate of central and southern Europe was considerably colder, and became subsequently modified, so as in part to cause the retreat of this animal to the northern regions.

In the bone caves of Dordogne, investigated by MM. Lartet and Christy, the most abundant animal was the reindeer, which evidently formed the principal article of food of the cave-dwellers, and, together with the ibex and the chamois, afford evidence that a considerable change of climate has taken place, for the former animal could not now exist in the south of France. "These caves are particularly interesting, because, so far at least as we can judge from the present state of the evidence, they belong to M. Lartet's reindeer period, and tend, therefore, to connect the later, or polished-stone age, with the period of the river drifts and great extinct mammalia, a period about which we had previously very little information." *

The discovery, in the early part of this year, of a human skeleton in one of the great caverns (Baoussé-roussé) of the Italian frontier, has again drawn attention to the subject, and excited much public curiosity at Mentone when announced in "Le Courrier de Menton," of April 7, with a plate of "Le Troglodite de Menton," from which journal the following notes are abstracted.† The cave is one of a series which occur in a compact limestone, and are known as *les grottes des roches rouges*. The caves are from 50 to 150 feet from the sea, and 40 or 50 feet above it, and all open to the south.

The discovery was made by Dr. E. Rivière, who has been appointed by the French Government to examine and study the palæontology and prehistoric period of Liguria. After obtaining an immense quantity of bones and teeth of bears, gigantic stags, hyænas, rhinoceros, and other animals, from the neighbouring quarries, Dr. Rivière commenced the exploration of the caverns. The cavern above alluded to is near the line of railway from Mentone to Vintimille, and the skeleton was found beneath a layer of earth several yards in thickness, and is in a very fine and remarkable state of preservation,

* Sir J. Lubbock, "Prehistoric Times," p. 245.

† The Plate has been reproduced from the "Geological Magazine" for June, whose Editors kindly lent it.

which may be possibly due to the nature of the earth in which it was imbedded and the continued dryness of the spot in which it was placed. The skeleton, which is that of an ordinary-sized man, is entire, with the exception of the ribs, which have been broken by the pressure of the superincumbent earth. The teeth and lower jaw are in a good state of preservation ; the skull differs from the rest of the bones in being of a deep brick-red colour, and the part of it resting on the ground is broken. The legs crossed in a natural position, and the arms folded near the head, seem to infer that the man to whom they belonged died in his sleep, and was carefully covered over without disturbing the earth beneath. A great number of small shells, similar to those living in the adjacent sea, and deer's teeth, all pierced with a hole, were close to the skull, leading to the belief that they were either twined in the hair or formed part of a head-dress. Round the skeleton were found a great quantity of stone implements, as scrapers, chisels, and axes, and also bone needles ; the form of the latter seem to have been produced by having been rubbed down on some hard substance. Associated with these were bones of animals, and, among others, the lower jaws of herbivora. Behind the loins there was a stone, also one behind the head, and between the latter were two of the largest stone implements which have been found in these caves.

Mr. M. Moggridge, who is well acquainted with the locality, and has visited the cave, has kindly furnished the following points as most important in reference to the skeleton :—

1. The rock in which the cave is situated is Oolitic or Jurassic limestone, which is very cavernous.
2. The cave is narrow and lofty, and deep.
3. The floor at the mouth is 9 ft. above the bottom, but in the interior the depth is much greater.
4. No remains of the extinct animals (*Ursus spelæus*, &c.) occur above the skeleton.
5. I believe it was a case of interment—of a person of some consequence—of the stone age ; not early in that period.

Mr. G. W. Nicholl, who has also examined the cavern, states : "To my mind the skeleton proves clearly a case of burial ; the stones at the back and in front of it showed this pretty clearly, for they were evidently so placed by design, as if to roughly mark out the place of sepulture. M. Rivière did not seem to think much of them, for he had removed them all before the skeleton was photographed. The sketch in the paper shows the stones at the back very truly, but it does not show those in front, which were placed further from the body and more irregularly than those at the back. If, then, the man was buried,

he might or might not have been co-existent with the extinct animals of whose bones so many were found in the cave earth in which the skeleton was interred. But close by, in front of the cave there was, as has been suggested to me, irresistible evidence that man lived in those caves (there are four or five of them) at the same time that animals now extinct were living in the neighbourhood. For in front of the cave is a *talus*, formed of breccia fallen from the cliff above. The stones forming this breccia are as sharp and angular as when they fell from the cliff, and they are cemented by lime and iron into a hard conglomerate. In this conglomerate, whilst making the railway cutting two years ago, were found numerous implements of flint—knives, spear and arrow heads, and cores of flints, from which these had been broken off, also bones of animals now extinct, and bones of animals now existing. Now as the stones forming the conglomerate are so very sharp and angular, it seems to me to be very conclusive evidence that they are lying where they first fell, and that the bones and flints amongst them are also lying just where they were thrown by the inhabitants of the caves above."

The report of Dr. Rivière will no doubt lay all the facts before us, as to this and the other caves, to which attention had been drawn by M. W. De Suiram, in 1869, and also by Dr. Falconer, who, in 1858, after examining at Nice the brecciated mass of human bones discovered near St. Hospice, visited the Rocco Rosso caverns near Mentone, which had previously yielded such abundant relics of long-continued human occupation, upon the exploration of M. François Forel.

In connection with this subject Mr. John Evans, in his recent work on "Ancient Stone Implements," June, 1872, states that the difference in the faunas of the palæolithic and neolithic periods is of great importance, as affording some guide in judging of the antiquity of human remains when found in caverns without any characteristic weapons or implements; such, for instance, as the human skull cited by Mr. Boyd Dawkins as having been found in a cave at the head of Cheddar Pass, in Somersetshire. For it must never be forgotten that the occupation of caves by man is not confined to any definite period; and that even in the case of the discovery of objects of human workmanship in direct association with the remains of the Pleistocene extinct mammals, their contemporaneity cannot be proved without careful observation of the circumstances under which they occur, even if then. Another point may also be here mentioned, namely, that where there is evidence of the occupation of a cavern by man, and also by large carnivores, they can hardly have been tenants in common, but the one must have preceded the other, or possibly the occupation by each may have alternated more than once.

REVIEWS.

MAN'S ORIGIN.*

THERE are some who would doubt the propriety of translating a work like the present one into the English tongue, and we almost fancy that the editor is to some extent of this class. But for ourselves we do not perceive its disadvantage. Indeed, on the other hand, we approve of the effort to introduce into our language so fearless and outspoken and honest a labour as that which Dr. Büchner has performed. In regard to the editor, we think he has been engaged in a task which must in every page have run hard against his conscience; and we wonder therefore that, if he did perform a task which must have so completely gone foul of his ideas, he did not conceal his name, and so have, to a certain extent, prevented the injurious influence—injurious at least to the author's views—which must follow a preface in which he states that he "is by no means inclined to accept all the results at which Dr. Büchner has arrived." Indeed, it seems us that Mr. Dallas must have found almost all of Dr. Büchner's views unacceptable; for it certainly appears that the author's one distinct and clearly-drawn conclusion is, that there is no such thing as a Deity, and his other, that man's existence comes to an end as soon as his life is finished. However, we must thank Mr. Dallas for having given us an excellent and clear translation of a most interesting and forcibly-written book, in which the author puts forward his views as clearly as he did in his former work, "Force and Matter," but without any of that offensive manner which created for his previous essay so many fierce opponents. Further gratitude is due to Mr. Dallas for not inserting a number of sharp, opposing foot-notes, showing what parts of the work he especially objected to. The book contains nothing that will be new to those who have followed minutely anthropological science for the past ten years, though it certainly contains, in a pithy and masterly style, nearly everything that has been achieved by the English and foreign *savants* in that space of time; but to the general reader, or to the dabbler in science, it offers a masterly summary of the facts, reflections, and ultimate conclusions that have been put forward in regard to man's origin during the period referred to. Indeed, in this respect we know of no volume that can be at all compared with it, not only for the facts it contains, but for the scholarly shape in which these facts have been put together and laid before

* "Man in the Past, Present, and Future," from the German of Dr. L. Büchner. By W. S. Dallas, F.L.S. London: Asher & Co., 1872.

the reader. Of course, in such a work the notes and references must be manifold, and we think that the method of placing them at the end of the volume both tends to lend the pages a better shape and to make the notes more carefully studied by the reader who refers to them.

The author regrets that his book had almost gone through the press before Darwin's celebrated volumes on "The Descent of Man" had made their appearance; but he congratulates himself on the fact that he has arrived, without the great English naturalist's aid, at practically the same conclusions. Dr. Büchner, of course, traces man through the different tribes which gradually, step by step, lead down almost to the brute. He gives numerous quotations from travellers of repute, showing the low mental, moral, and physical condition of these savages, and then he points out how, even in their anatomy, they approach the apes. Then he gives the numerous instances which have, during the past ten years, been brought forward regarding pre-historic man, and he shows conclusively that in many of the examples the skeleton—especially the skull—possessed, in a marked degree, characters that are now not exhibited by any living creature, save certain of the quadrumana. In all his observations on this point of course it is impossible to go against him. We mean, that for any man at all conversant with anatomy, such a line of reasoning as would be opposed to Herr Büchner would be in the highest degree absurd. Here you have man possessing the same skeletal formation, or nearly so; the same heart, lungs, thyroid, diaphragm, liver, stomach, and intestines; same kidneys, same spleen, same pancreas, same organs of reproduction, nearly the same hands, and a closer allied brain than that possessed by any other mammal. And notwithstanding all these important details of anatomy you are asked to believe that man was made from clay—though he hardly contains a particle of its most universal constituent, alumina. Assuredly, all reasonable men will go in with Dr. Büchner, and will believe that man must have come from the apes, a doctrine which has so much to support it, rather than the other view, which is merely a questionable hypothesis of the almost untranslatable Holy Writ, and which, besides having nothing to support it, is opposed on the most important chemical grounds to which we have alluded.

There is only one instance in which it seems to us that the author is incorrect, and that is a case that may fairly be forgiven him, for the best authorities in France are of the same opinion. It is in reference to the Moulin-Quignon jaw-bone, which he in common with the Frenchmen pronounces to have been a fossil. We cannot regard the bone as in the least degree antique. It seems to us unquestionable that it was a bone of recent formation, which was imposed upon the French savants who obtained it. In our opinion Mr. Busk's method of examination was the only correct one. He sawed the bone through, and obtained the same peculiar smell which is so well known to those who manipulate recent bones. It is utterly out of the question that this peculiar smell could have been obtained from any but a recent bone, and therefore we regard the Moulin-Quignon jaw-bone as representing a scientific canard. However, there are other specimens whose antiquity is beyond question, and of them Herr Dr. Büchner gives a

good and clear account. Let us take the Neanderthal skull and La Naulette jaw as examples:—

"The most important of these remains is the celebrated *Neanderthal* skull already mentioned * * * which Huxley describes as the most ape-like of all the human skulls that he has ever seen, and of which he says that in its examination we meet with ape-like characters in all parts, and also that it has the greatest similarity with the existing Australian skulls, and with the ancient Borreby skulls. Huxley also states that this skull is by no means an *isolated* phenomenon, but that it is only the extreme term of a long series of bestial, or, at least, very lowly-developed human skulls of the past and present periods." Again, on the same subject, the author writes: "in the year 1866 a fragment of a human jaw with very remarkable and animal characters was found by the indefatigable Belgian cave-explorer, Dr. Edward Dupont, in the *Trou de la Naulette*, a lone cave situated on the bank of the little river Lesse, not far from the village of Chaleux. It was in a deposit of river-loam, covered with a layer of stalagmite, and at a depth of about four metres. The most remarkable of its characters, besides the comparative thickness and rounded form of the bone, and its elliptical dental curve, *is the almost entire absence of the chin.* The projecting or prominent chin is so distinctive a character of man that Linné, the great lawgiver of systematic zoology, could name no better bodily distinctions between man and animals, than the upright position and prominent chin of the former. In animals, instead of projecting the chin retreats, and the jaw of La Naulette holds an intermediate position between the two; where the projection of the chin ought to be, it exhibits a line descending perpendicularly. Moreover the cavities destined for the reception of the canine teeth are remarkably wide and large as in animals, although the canines themselves are closely contiguous to the incisors, and *not* molars, and the jaw is thus shewn to be undoubtedly of human origin. But what is still more remarkable than this is the circumstance that the three hinder or persistent molars present exactly the same relative sizes as is usual in the anthro-promorphous apes. Thus whilst in the higher races of man, the three true molars are so arranged that the first is the largest and the last or hindermost the smallest, we find in the dentition of the lower races, such as the Malays and Negroes, that all the three molars are of equal size, and throughout larger than usual. But in the anthropoid apes the first true molar is the smallest, and the last the largest, and this is the case also in this fossil human jaw, the last or hindermost molar of which even appears to have possessed five roots. To all this may be added that the inner surface of the jaw at the point of the so-called suture or symphisis, behind the incisor teeth, forms a line obliquely directed upwards, and consequently leaves no doubt as to the prognathism of its former possessor."

We fancy that the above quotation will suffice to show that the author has not dealt in a slovenly fashion with his book, but has been at care in dealing with points of evidence; he gives them fully, minutely, and thoroughly. The other portions of the book are alike, and they all bear the author along to the conclusion regarding man, which he expresses in his last chapter. We must now leave the work in our readers' hands, begging of them to read it carefully, and not either from enthusiasm or from malice to do this author's efforts any injustice.

THE FALLACIES OF DARWINISM.*

THE opponents of Darwinism are numerous enough, but we may add by no means intelligent enough to do anything but help to extend Mr. Darwin's views by bringing very feeble arguments against Darwinian views before people who are ignorant of the whole subject, but who are intelligent enough to perceive fallacies when put before them, and who, once they are called upon to consider whether man has descended from the other animals or has been *separately* and *similarly* made, very naturally conclude that the former is by far the most probable of the two. There is another quality, too, which the opponents of Darwinism possess, and which certainly by no means tends to render their arguments more convincing, and that is the faculty of abuse. In every chapter of their works there is a summing up in which they express their astonishment at the very existence of unbelievers, and that too in terms usually of the very coarsest abuse that can be conceived. How unlike the whole number of Mr. Darwin's or Mr. Wallace's books. In no single passage do they attempt to revile—as indeed they might most easily—the abortive and often absurd arguments of their opponents. Dr. Bree's is a book of the ordinary class, an abusive review of Mr. Darwin's theory, as unlike Mr. Mivart's able essay as anything possible to conceive, and by no means to be placed alongside it or compared with it in any respect. Yet the author has not been without some reading on the point. He has found out the names of two or three naturalists of repute who do not hold the Darwinian doctrines, but he is by no means to be considered as having Professor Owen on his side, though he evidently considers that the distinguished naturalist leans towards his views. We may not call Professor Owen a Darwinian, but if we could take Mr. Darwin's name from the doctrine, we doubt not Professor Owen would hold to it, for indeed he alleges that he put forward similar views quite thirty years since. Of the other names which Dr. Bree has got, we fail to see any of special importance in a zoological respect except Agassiz, though of course we could ourselves name some few more whom possibly Dr. Bree has never heard of. But if Dr. Bree were to ask to us to name those eminent masters of zoological science, which, at first opponents of Darwinism, have since become its firm supporters, he would doubtless be a little astonished at the multitude of European and American names we could put before him. The book is simply a series of bitter attacks on the views of Mr. Darwin, Professor Huxley, and the leading evolutionists of this country and the Continent. Of course there is some reason given for not holding particular theories arising out of the main doctrine, but these are indeed few, and not particular; while the arguments of the author are of the old-womanish kind, and invariably end with allusion to the Creator, and an assertion that therefore the doctrine in question must be absurd, because it is out of harmony with the author's particular religious belief. Let us take a paragraph from the chapter in which the author exhibits his knowledge of Van Baër's work. Here, after denying Mivart's view, he says :—

* "An Exposition of Fallacies in the Hypothesis of Mr. Darwin." By C. R. Bree, M.D., F.L.S. London: Longmans, 1872.

"We *know* that the organic and inorganic worlds have been formed by a thoughtful reasoning being; but the 'how' or 'why' are hidden among the mysteries of Omnipotence . . . ." and further, Dr. Bree having nearly exhausted his vocabulary of abusive epithets against Mr. Darwin and those who believe with him, says of the Darwinian theory that "if proved in every point to be true, it would still leave the fact of special creation in all its wonderful mystery. The organic *cannot* be formed from the inorganic; nor could the organic, even if it were so formed, be endowed by any physical force with the laws and properties of life. Go on still in speculation, and I ask whence the inorganic—its beginning, its ending, its grand and inexplicable laws, which the physicist in vain attempts to correlate with the vital? whence gravitation, and what? the sidereal system and its movements? the spirit that breathes, through illimitable space, and lives through an eternity of time?"

To a shallow mind it may seem a very grandiose speculation, but suppose we go further, and assuming the existence of the Creator, ask the author whence did he come, what answer will he give? We dislike this kind of speculation intensely, it carries the narrow-minded person off his balance, but to the man of thought it is simply the verbose and shallow wanderings of a mind which has not yet arrived at the conclusion that the whole subject he has been talking of is most probably for ever removed from the speculations of man. It is an absurd wandering from the matter of animal life and from the very simple though yet unproven question, whence comes man, from the apes or from mud?

AIR AND RAIN.*

HERE we have a book which is new in its subject and mode of treatment; and it is a large one too, extending over more than five hundred pages. What shall we say of it? Is it good or bad, too long or too short; too much confined to facts without deductions; or given to over-extended generalisations, to the exclusion of accurate observation? We must answer these several questions by stating that the work appears to us a good one, but unsuited either to the general reader or to the scientific man. It is unsatisfactory for the former because of the quantity of tabular matter which is introduced into its pages, and to the latter because the author indulges in a great series of observations which are not clearly put, and which are intended for general audiences. Still, it seems to us to be a good book: it is one with which we have hardly a single fault to find; and we give the author the highest praise for the manly and fearless manner in which he has in all cases spoken his mind, and for the intense labour which the work must have entailed by reason of the numerous analyses which it contains of air and water made over every portion of the kingdom. It would be a labour without end to attempt anything like a full notice of the author's various and manifold researches. We shall

* "Air and Rain: the Beginnings of a Chemical Climatology." By Robert Angus Smith, Ph.D., F.R.S., F.C.S. London: Longmans, 1872.

content ourselves with a few remarks on the author's opinions relative to that important question whether drains, as we have in London, or middens, which some of the manufacturing towns possess, are, all circumstances considered, the most convenient and healthy. Thus we shall leave the various analyses he publishes out of the question, and shall pass his drawings of the products found by the microscope in the air of certain localities altogether aside. The latter we do more especially because the author has given us little or no idea of the enlargement of the specimens, and we fancy that in most cases low powers of 400 to 600 diameters have been most usually employed. We shall merely notice, too, the fact that he has been at pains, in conducting his analyses, to employ the most recent analytic methods—excluding Dr. Frankland's, and including Wanklyn & Chapman's method. Neither shall we deal with the author's views on the subject of meteorites, which we deem on a par with Sir William Thompson's notion, which was given to the British Association. But it seems to us that Dr. Smith's observations on the subject of crowding are most valuable, and we hope they will be read and carefully remembered by all those who have to do with the building of our towns and the general management of house property. Dr. Smith says, "There is a want of willingness to pull down dangerous property, but a readiness to rush forward to save the life of the greatest criminals. Reason is out of the question in the matter; we are misled by an uneducated feeling. We like to save property, forgetful that deadly weapons and poisons are subject to peculiar laws, and their indiscriminate use is forbidden to the nation. Houses that produce death are not property; as well might a man claim his debts as such. If a man sells unwholesome meat, the law interferes; if he sells the use of a room with fever in it, the nation seems not to complain. . . . The time must come—and the sooner the better—when it shall be enacted that no land shall contain more people per acre than we know, by experience in several places, can live healthily thereon. The same thing must be said regarding houses . . . because of the degradation of some of the population." On the midden versus sewer system, the author's opinions clearly lead us to this, that where water is abundantly flushed through the sewer, it is the best; otherwise the midden is infinitely superior, and causes much less death to the population. Dr. Smith says, he has "come long ago to the conclusion, that the water-closet is one of the greatest of luxuries invented in modern times; but also thinks that the midden is better than the bad sewer . . . The question is not a simple one for a *yes* or *no*, but an extremely complicated one, where many conditions must be balanced. This experiment, however, is desirable—the examination of the air outside of the houses of a sewered town and a midden town. I feel almost confident of the answer; indeed, the analyses in this volume may be said to give it, because the backs and fronts of the houses of a midden town give different airs—the backs giving worst; whereas the backs and fronts in a water-system town cannot be different, one would suppose. . . . I think it probable that, as matters are now conducted, the water system will be the worst in all houses not large enough to have sufficient separation. . . . We come to this, that the danger is outside in the midden system, and inside in the water system, where the danger does exist."

Dr. Smith's remarks are judiciously cautious, but they are to the point; and the multitude of analyses by which they are accompanied bear them fully out. Altogether, we are well pleased with the book, which, with a few deficiencies in style, is an admirable essay on a very difficult question.

MILLER'S CHEMISTRY: PHYSICS.*

THE fifth edition of Part II. of this excellent treatise on Chemistry is now before us, and we may just say a word or two about it. We remember well enough the appearance of the first edition of this work when we were at college some fifteen years ago. It was an excellent introduction to chemical physics—clear and intelligible to the lowest intellect, amply illustrated, and printed in a large and bold type. In what, then, does the present work differ from the first one? Well, it is nearly twice the size, contains a bulk of matter which renders it a terrible book to the readers of the first edition, and the matter is excellently arranged, and is all that the mere student of chemistry can require; indeed, it is much more than an ordinary chemical student requires. Still, there are some parts which ought to have additions made to them, and some which, in our opinion, might as well have been cut out. But, doubtless, that was out of the question in editing a work like the present one. Mr. McLeod has, we think, performed his task with extreme caution and excellent good taste; and he has introduced a quantity of matter in relation to spectrum analysis, and to the question which has lately occupied the Chemical Society—that of atomicity; thus bringing the book up to the time, so that it forms an excellent manual of physics for the chemical student.

GEOLOGICAL SURVEY OF OHIO.†

DURING the time occupied by the geological survey of the State, the Chief Geologist is required to make an annual report of the progress of the survey, with such necessary illustrations as may exemplify the same; and the present volume is the result of the field and other work for 1870. The determination of the geological structure of Ohio was not only important as bearing on the character, variety, and distribution of the mineral riches of the district, but also as showing the connection of the geological features of the country lying between the Atlantic and Mississipi. Already the number of formations known to exist in the State has been nearly

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* "Elements of Chemistry, Theoretical and Practical." By William Allen Miller, M.D., D.C.L., late Professor of Chemistry in King's College, London. Revised by H. McLeod, F.C.S. Part II. Chemical Physics. 5th edition. London: Longmans, 1872.

† "Geological Survey of Ohio: Report of Progress in 1870." By J. S. Newberry, Chief Geologist, and Assistants. 8vo., pp. 578, with maps and sections. Columbus, 1871.

doubled, and their general structure determined; but the final report will embody all the local details of the different counties, with the needful maps, plans, and sections necessary for their proper illustration.

From the present report it appears that the coal measures occupy a larger portion of the surface area than any other formation; and, from the number of economical substances associated with them, their character and distribution has been carefully studied. The first part comprises a sketch of the *lower* coal measures in north-eastern Ohio, by Mr. Newberry; from which it appears they contain seven or eight workable seams of coal, all of which lie below the celebrated Pittsburg seam, and include, in fact, the most important coal strata of the State. The coal measures do not form one symmetrical basin, but several troughs in a general way parallel with the axis of the great one of which they are parts. On the east side of each of these subordinate basins the strata rise, or are horizontal, and the easterly dip is neutralised; so that on the east line of the Columbiana county the section of the hills is nearly the same as that found on the banks of Killbuck, one hundred miles west—the average dip in this interval being not more than three feet to the mile. In tracing the strata from the western margin of the coal-field to the Pennsylvania line, some of the coal seams disappear, and others come in; and local changes are discoverable both in the development and purity of the different seams of coal and iron. The coals are of various qualities. The upper seams are well adapted for the generation of steam; below them are the cannel coals, which, although the difference in heating power is not great, contain a larger amount of ash than the English Wigan cannel; still, however, they can yield a large volume of good illuminating gas. The most valuable seam is the lowest, or Briar Hill coal, both for its thickness, purity, and being well adapted, in the raw state, for the smelting of iron ores. With regard to surface features, Mr. Newberry points out that the rivers, as the Killbuck and Tuscarawas, run in parallel synclinal valleys, and that the folding of the strata which formed these subordinate troughs and ridges in the coal basin first gave direction to the drainage streams of the region, and which lines of drainage have retained, through all subsequent mutations, the directions thus given them; and that this direction, as well as that of the main tributaries of the State, have been determined by the same causes that produced the great folds of the Alleghany mountains.

The other portion of the volume contains a report of labours in the second geological district, by E. B. Andrews, accompanied by maps of grouped sections, showing the strata of the lower coal measures in detail. The geology of Highland county, by E. Horton; which county contains a more extensive geological series than is to be found in any other county of the State, as it includes the Lower and Upper Silurian, Devonian, and Carboniferous formations, besides some Drift deposits and evidences of glacial action. The agricultural survey is by J. H. Klippart, and is intended to give a brief exposition of the physical and chemical characters of the various soils, their origin, the part they play in the growth of vegetation, the source of their fertility, and the theory of their impoverishment. The chemical report, by T. G. Wormley, is more specially devoted to the analyses of the coals, iron ores, furnace-slags, fire-clays, soils, limestones, and the pro-

cesses by which they were effected. Sketches of the geology of Geauga and Holmes counties, by M. C. Read, and of Williams, Fulton, and Lucas counties, by C. K. Gilbert. On the present state of Iron Manufacture in Great Britain, by W. B. Potter. And, lastly, some interesting notes on the present state of the Steel industry, by H. Newton.

MAN'S ORIGIN AND DESTINY.*

A SINGULAR book this; one of its own kind, full of learning, and rather addressed by its style to those who are engaged in similar pursuits than intended for the populace. It is essentially a deistical work, not Atheistic, for the author concludes by expressing a belief that there is a Deity, and that we shall hereafter, when we have passed from this world, enjoy a life of intense happiness. However, it is not that portion of the book which contains the speculations of the author that is the most important. Especially those chapters—and they are numerous enough, as the book extends over more than 770 pages—which deal with the scriptural account of the Lord, and the various other writings, some of them very old, and others the works of the holy men of the middle ages, are full of interest, for they let us into secrets that were unknown to all but those who have made the biblical records the study of their lives. We cannot attempt to review the author's efforts for they are to be judged alone by those few Eastern scholars who can thoroughly follow the writer throughout. Still we see beyond question that he is quite correct in his onslaught on the supposed divine origin of the Pentateuch, and other biblical writings. Starting with Origen's question, he asks, "What man of good sense will ever persuade himself that there has been a first, a second, and a third day, and that these days have each of them had their morning and their evening, when there was as yet neither sun nor moon nor stars? What man is there so simple as to believe that God, personifying a gardener, planted a garden in the East? that the true tree of life was a real tree, which could be touched, and the fruit of which had the power of preserving life?" It is certainly too true that the great mass of people believe the tales of Moses, whilst they would not think of believing them if they were in the writing of a Greek philosopher, or a Rabbi, or a Mahometan, because they believe Moses to have been inspired. But we certainly agree with the learned author, that when we see that these books of Moses are full of repetitions and contradiction, we must give up all notion of their being inspired. A few of these contradictions may not be known to all, so we give them from the author: "The hesitation of Moses when he received the order to deliver the Israelites is mentioned twice in different terms. The miracle of the cloud on the tabernacle is related twice with different particulars. Jacob is made to be 84 years old when he took Leah to wife, while Dinah was scarcely seven years of age when she was violated by Shechem, and Simeon and Levi

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* On "Mankind, their Origin and Destiny," by an M.A. of Balliol College, Oxford. London: Longmans, 1872.

were scarcely twelve and eleven years old when they ravaged a city and put all the inhabitants to the sword. Some of the laws are mentioned twice, and each time they are different in it." These are but a sample of a host of such errors which the author dwells on. The book throughout is full of interest, and the plates representing various objects, chiefly from Eastern temples, are remarkably interesting. Altogether we are much pleased with the book, which is, however, of a class unsuited to our pages. We wish the author had given his name, for then we could more thoroughly thank him for the good efforts which he has made, and for which, even though he be unknown, we thank him.

RADIANT HEAT.*

HERE we have a book which cannot be termed a popular work in any sense of the word, but which may be read, we think, with interest even by those who have little knowledge of the subject, but who have minds capable of taking up questions of this kind. It consists of a series of memoirs chiefly on the subject of radiant heat. These have for the most part been contributed to the "Philosophical Transactions," and the "Philosophical Magazine," and they together form such an admirable essay on the whole subject that we fancy Professor Tyndall did rightly in reproducing them. Furthermore, they will certainly tend to let many understand who were ignorant before, of the nature of the dispute between the author and Professor Magnus. They will, from this point alone, be of service, for they will show how convincingly strong are the arguments Professor Tyndall advances, and how admirably his experiments have been devised and carried out. To our minds, individually, the most popularly interesting paper of the whole is that on calorescence, which is headed by those aptly-chosen lines from Lucretius. We see no reason why any of our readers should be unable to follow the author through this portion of his book, which is written in Professor Tyndall's usually eloquent, forcible, and clear style, and which is amply illustrated by woodcuts. Altogether the book is of course an excellent one, and we sincerely hope its sale will fully recompense its publishers.

GANOT'S POPULAR NATURAL PHILOSOPHY.†

WE do not know whether this book is to be regarded as superior to all our English books for beginners in natural philosophy; indeed, we think ourselves that it is not. Still, it must be admitted that it is a very

* "Contributions to Molecular Physics in the Domain of Radiant Heat," by John Tyndall, LL.D., F.R.S., Professor of Natural Philosophy in the Royal Institution. London: Longmans, 1872.

† "Natural Philosophy for General Readers and Young Persons," Translated and Edited from Ganot's "Cours Élémementaire de Physique." By E. Atkinson, Ph.D., F.C.S., Professor of Experimental Science in the Staff College. London: Longmans, 1872.

good book of its kind, and that it is by the same author as that of, perhaps, the best-known manual of physics. It is carefully prepared by the English editor, who has discharged his task of translation very well indeed; there being, as far as we have seen, an utter absence of any of that peculiar French idiom so objectionable in a translation. The book is nicely printed, and its illustrations are, in our estimation, the very best part of the whole volume. These are excellently done, and are no less in number than 414. We fancy we notice a few errors here and there in the optical illustrations; but they are defects by the absence of special rays rather than otherwise. In the next edition, too, it would be as well if Dr. Atkinson would look it over carefully, so as to avoid those errors which are absolutely inevitable in the first. We would point to one, for example, that certainly appears so to us. It is on p. 339, eight lines from the bottom. The author, speaking of the immense value of the spectrum analyses, says that "their extreme delicacy constitutes them a most valuable help in the *quantitative* analysis of the alkalies," &c. Surely he meant qualitative in this case. There are others also in the volume, but of much less importance. Altogether, the translation is very creditable to the editor.

ESSAYS ON ASTRONOMY.*

MR. PROCTOR has so long and so frequently contributed papers to our columns that we suppose our readers will think we are prejudiced in his favour. It is not so, however. We see Mr. Proctor's good qualities, and we see his faults also; but the latter are so very small in comparison with the former that they are almost insignificant. He has eminently the faculty—the too dangerous acquirement in the case of a scientific man—of writing. Let him take up any subject which he understands, and he is able to write there and then a long paper on the subject. Whilst most men would require to collect material, and, having done so, to arrange it, and, having settled the matter, would have considerable difficulty in committing their thoughts to paper, with Mr. Proctor it is done at once. This gives him a great advantage over his fellow-workers. But it has its disadvantages. It leads a man to contribute something to nearly every journal, and this is what Mr. Proctor does, and it is where, we think, he errs. If even it be granted that in the majority of cases he is but popularising that which he has aired in scientific arenas at an earlier date, he is, at all events, doing that which the majority of scientific men refrain from, and which is generally thought to give a man a character for light thinking rather than for serious heavy work. However, in the essays before us at present this idea will not occur to anyone. They are all serious papers, a few taken from our own pages, and others from various other journals, the majority of

* "Essays on Astronomy: a series of papers on Planets and Meteors, the Sun and Sun-surrounding Space," &c.; preceded by a sketch of the life and work of Sir John Herschel. By Richard A. Proctor, B.A., F.R.A.S. London: Longmans, 1872.

them appearing to be excerpted from the "Proceedings of the Royal Astronomical Society." All the papers are good, some of them indeed are excellent; but they have all come before scientific readers before, and are now addressed to the general public. The opening chapters are about the most generally interesting in the volume, as they deal with the life and labours of the late Sir John Herschel; but all will be found interesting and profitable reading.

BOTANY FOR BEGINNERS.*

WE think that Dr. Masters has not been guilty of a commercial mistake in publishing the book before us, the greater part of which originally appeared in the pages of the "Gardener's Chronicle." But altogether apart from the monetary aspect of the question we have reason to be grateful to the author for the trouble he has taken to write a genuinely popular work; a book, be it understood, which is absolutely devoid of the abominable twaddle of most popular treatises, but which is couched in language intelligible to every reader, which is so arranged that each difficulty is provided for, and each technicality is explained as the young student goes on, and which, nevertheless, does not keep the pupil for days learning the interminably dry details of the flower and stem in the abstract, but which plunges *in medias res* of botany at once. The first six chapters deal with various plants so common as to be accessible to every one even in this huge city, and the remaining four treat of the physiology and classification of vegetables. The illustrations are most of them novel to the student, and are nearly eighty in number. We certainly thank the author very heartily for so admirable a little book as the "Botany for Beginners."

ANTI-DARWINISTS.†

WE have before us two books whose whole aim is to show us the errors of the Darwinism doctrines, and the truth of the older views. We cannot agree with the conclusions of either. In fact, neither has a single argument to adduce that, resolved to its simplest elements, can resist the general doctrines of evolutionists; but the one is written in a true scientific spirit: we do not say that it has a great deal of science in its pages, but the spirit of science is there, calmness, coolness, patience. The other has not an atom of scientific value, and very little of mere literary worth either.

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* "Botany for Beginners: an Introduction to the Study of Plants." By Maxwell T. Masters, M.D., F.R.S. London: Bradbury and Evans, 1872.

† "The Higher Ministry of Nature, viewed in the light of Modern Science, and as an aid to advance Christian Philosophy." By J. R. Leifchild, M.A. London: Hodder and Stoughton, 1872.

"Esse and Posse: a Comparison of Divine Eternal Laws and Powers, as severally indicated in Fact, Faith, and Record." By H. T. Braithwaite, M.A. London: Longmans, 1872.

Both books we disapprove of because of the views they contain, but the one, that of Mr. Leifchild, we can commend to those who wish to read such a book, while for the other, which bears the name of a Mr. Braithwaite, we cannot say a syllable in favour of. In style it is affected, in reasoning fearfully shallow, in substance it has nothing upon its naked bones. It is, therefore, an empty, vacuous work, in no way to be recommended. On the other hand, Mr. Leifchild's work, though it is large, calmly written, dealing with its opponents in a truly Christian spirit, and above all things nicely written, is a poor argument against the thousand reasons which the evolutionists can urge against it. In any case, we commend it to the notice of our readers, but we cannot honestly recommend the empty book which Mr. Braithwaite has given us.

A MARVELLOUS CHART.*

TRULY a wonderful work that which gives in the space of an ordinary map—a circle of about one foot two inches across—every star which is to be found in Argelander's wondrous star-maps of the Northern Hemisphere. In other words, this map of Mr. Proctor's displays at once in the proportionate size and in their exact position, no less than 324,198 heavenly bodies. It is certainly a marvel of work, and is best studied by means of a good large reading lens. Indeed, without this it will look to many aged persons as simply a dirty circle, which has a filthier aspect in some parts than others. Besides this there is published with it a key map, in which the larger constellations of the Northern Hemisphere are distinctly represented, and the figures on this can be very readily distinguished in the photographic map—for it has been done by photography, as being more exact. Truly enough has Mr. Proctor said, that "to the general student of science the chart is chiefly of use in affording the means of enlarging his conceptions respecting the glories of the celestial depths. If he remembers that every one of the dots in the chart represents in reality a sun—a sun perhaps exceeding our own in magnitude and splendour—he cannot fail to be impressed with a sense of the grandeur of the stellar universe." This admirable map we, through some mistake, omitted noticing in our last number, and we beg to offer our ample apologies for the fact to both our readers and the author.

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* "A Chart of the Northern Hemisphere, on an equal-surface projection; showing all the stars in Argelander's series of forty full-sheet charts—324,198 in all, with a key map on the same projection." By R. A. Proctor, B.A., F.R.A.S. Manchester: Photographed and Published by A. Brothers, 1871.

SHORT NOTICES.

Conversations on Natural Philosophy. By Mrs. Marcet: revised and edited by her son, Francis Marcet, F.R.S. 14th edition. London: Longmans, 1872. Those who like this book—and we believe it can number its admirers by thousands—will be pleased to find a new edition of it. It is, so far as we have examined it, tolerably accurate, and it is fuller than many text-books on the subject. The illustrations are both numerous and good, and the chapter on spectrum analysis is singularly excellent.

On the Antagonism between the Actions of Physostigma and Atropia. By Thomas R. Fraser, M.D., Lecturer on Therapeutics at Surgeons' Hall, Edinburgh. Edinburgh: Neil & Co., 1872. This is a work reprinted from the "Transactions of the Royal Society of Edinburgh." It contains nearly 200 pages, accompanied by numerous charts, and is certainly the most voluminous account which has ever been published upon the action of two drugs. It has an interest of the highest kind, not for the general reader, but for the medical man; and we hope there are few who are interested in the opposite action of drugs who will not carefully study Dr. Fraser's important results. The general conclusion at which the author has arrived from hundreds of experiments, though not exactly novel, is nevertheless of importance. He says (page 617) that "the conditions of the experiments, and the symptoms that were observed, render it certain that atropia prevents the fatal effect of a lethal dose of physostigma, by so influencing the functions of certain structures as to prevent such mortification being produced in them by physostigma as would result in death."

The Sun: Ruler, Fire, Light, and Life of the Planetary System. By R. A. Proctor, B.A. 2nd edition. London: Longmans, 1872. We need only say that this volume has gone to a second edition, and has sold more than 2,000 copies in the year. The present issue contains several new illustrations and some important additions.

Popular Physiology. By E. D. Mapother, M.D. London: Longmans, 1871. This book was not noticed, through some omission, in our recent number. It is a very small book of over 100 pages. Contains a good many illustrations. It is only intended for the most elementary possible of classes.

The Earth's Crust: a Handy Outline of Geology. By David Page, LL.D., F.G.S. Blackwood & Sons, 1872. This is the sixth edition of a good elementary outline of the science. It is just the sort of book for a class-book in school-rooms.

The Year Book of Facts in Science and Art. By John Timbs. Lockwood & Co., 1872. This contains just the usual amount of matter, with the usual degree of printer's blunders or other erroneous matter. We cannot at all approve of so badly edited a volume.

The Fairfield Orchids cultivated by Messrs. James Burke & Co. Bradbury & Evans, 1872. This is, of course, a publication got up for the purpose of selling the orchids of this firm. We believe that Mr. Leo Grindon had a share in its production.

The Stone-Age in New Jersey. By Charles C. Abbott, M.D. Salem, U.S., 1872. This is unquestionably the best paper and most elaborately illustrated which has appeared on flint weapons in any part of the world. It is admirably done, and we are glad it has been reprinted from the "American Naturalist," in which it originally appeared in last March or April.

Optical Illusions Explained. By A. B. Lacy, 1871. Is simply an explanation which exists in every treatise on natural philosophy in existence.

The Testimony of the Rocks, &c. By T. Callard. London: Elliot Stock, 1872. The whole matter is explained in about twenty-five pages of the smallest duodecimo. The author has been fortunate, we must say.

The Existence of Projectile Forces in Nature. By John Parker. New York: Wiley, 1872. The author simplifies the whole of astronomical questions, in twelve pages 8vo. ! !

The Hygiene of Air and Water. By W. Proctor, M.D., F.C.S. London: Hardwicke, 1872. This is a sensible little book on two important points in Hygiene. We recommend it to our readers' notice.

Human Progress in Medical Education. By W. Aitken, M.D., Professor of Pathology in the Army Medical School. London: Griffin & Co., 1872. This is a very capital address, given by the Professor of Pathology at Netley. It will repay attention. Dr. Aitken seldom speaks without saying something to the point.

SCIENTIFIC SUMMARY.

ASTRONOMY.

PROPOSAL *for new National Observatories.*—Col. Strange has called the attention of the Royal Astronomical Society to the insufficiency of existing national observatories, in respect of what may be called the physics of astronomy. He proposes that Government should be applied to for money to found other observatories than those at present in existence, and that in these new observatories the study of the physical features of the sun and moon, planets, comets, nebulæ, and stars, should be prosecuted systematically. At present the feeling in scientific circles is in favour rather of an extension of the work done at existing observatories, than of the foundation of new observing stations.

The Eclipse of December last.—In the last number of this journal will be found the chief particulars of the observations made upon the recent eclipse. It was hoped, however, that at the March meeting of the Astronomical Society full reports would have been made by the English observing expedition, and that many details of interest would then be announced. To the astonishment, and indeed to the disgust, of the crowded meeting which assembled on the strength of this expectation, no report whatever was then made! Nor was any report delivered at the April or May meetings! The silence of those whose duty it was to report the proceedings of the English expedition has naturally given rise to much comment, by no means favourable to the nominal chief of the expedition; the more so that the said head of the expedition has made free use of the information handed in to him by his fellow-observers, as well in lectures as in papers published at a price. It is felt that (unless there be some explanation as yet unpublished) it is altogether unworthy of a student of science to refrain from making due announcement of discoveries effected by the aid of Government money, and by the skill of fellow-workers who have unsuspectingly entrusted the records of their work into his hands, unless when the announcement can be so made as to be repaid with so many pounds, shillings, and pence. We trust for the honour of British science that some explanation may yet be forthcoming.

In marked contrast to the action of the reputed head of the English expedition, is that of Col. Tennant. So soon as he had reached England, he laid his statement before the Royal Astronomical Society, and submitted to

examination the splendid photographs which were obtained at Dodabetta. It should be noticed in passing that the photographs, scarcely (if at all) less perfect, obtained by Mr. Davis, who at Lord Lindsay's charge accompanied the English expedition, escaped as by a miracle from the tight grip which laid hold of everything else obtained by that party,* and were thus available for comparison with those of Col. Tennant. Nothing could possibly be more convincing than the evidence given by either series separately, and by the two series when taken together. The details of the corona in these photographs are very numerous, and wonderfully distinct; *all of them can be recognised in all the photographs.* We cannot wonder that even those who had been most opposed to the solar theory of the corona have at last given in their adhesion to it. We do not say that all of them have, for that would be expecting too much, human nature being as it is. But that any of the opponents of that theory should have adopted it, speaks volumes. Now Dr. De la Rue, who, it will be remembered, had urged arguments in favour of Oudemann's theory, which may be called the lunar theory of the corona, admitted, with a candour and frankness which did him infinite honour, that the photographs obtained last December demonstrate the solar nature of the phenomenon.

Densities of Jupiter's Satellites.—Mr. Proctor has called attention to the incorrect values of Jupiter's satellites which have found their way into our text-books of astronomy. These values have led to erroneous assumptions as to the condition of these bodies. The following table shows at once the incorrect values formerly adopted, and those which Mr. Proctor has calculated :

Satellite	Mass that of Jupiter =1	Density that of Earth=1		Density that of Water=1	
		Text-book value	True value	Text-book value	True value
I.	0·0000173	0·0202	0·198	0·114	1·148
II.	0·0000232	0·0302	0·374	0·171	2·167
III.	0·0000885	0·0698	0.325	0·396	1·883
IV.	0·0000427	0·0393	0·253	0·223	1·468

It will be seen that the errors are enormous, nor is there any recognisable explanation. For the density of the first satellite is given in our text-books at about a tenth of its true value, but that of the third at rather more than a fifth. One obtains something very like the text-book values, however, by supposing all the satellites equal in size to the first, and by reducing the thence estimated density to one-tenth of its value. Assuredly, supposing this to be the true explanation—or, indeed, whatever may be the true explanation—the error is a most monstrous one, to be repeated, as it has been, from one hand-book of astronomy to another. It may fairly be asked what confidence can be placed in such hand-books when blunders like these are suffered by their authors to escape detection.

Orbit of the Double-star Castor.—Mr. Wilson, M.A., of Rugby, enunciates the somewhat startling theory that the orbit of Castor is hyperbolic, "a form

* Probably because, being obtained at Lord Lindsay's expense, there was a private claim upon them. The public claim upon the observations of the members of the expedition could be ignored, it would seem ; but, fortunately for science, Lord Lindsay successfully urged his claim upon his own property.

of orbit," as he truly remarks, "which has not been shown to exist in the case of any binary system." It may be remarked that Sir John Herschel, nearly forty years ago, deduced the period 253 years, while Smyth obtained a period of 240 years. In the Monthly Notices for December 1845, Mr. Hind gives elements "entirely differing from those previously computed by Herschel and Mädler," the difference being "materially owing to the great influence exerted by recent measures at Mr. Bishop's observatory by Mr. Dawes." Mr. Wilson's result involves a somewhat startling advance in the same direction. He finds that the apparent orbit is part of an hyperbola of eccentricity 2·2. "The real hyperbola may be shown by a graphical construction to have an eccentricity of 3·16, and its line of nodes nearly coincides with the axis major." "If this orbit is correct, the angle of position will decrease to the limit 188°. It is now about 238°·12, according to the observations of Mr. Seabroke and myself," he adds, "and a little less by the interpolating curve—about 237°·85."

An Unsuspected Cause of Diffraction Phenomena in Telescopes.—Captain Noble makes the following remarks in the Monthly Notices for April:—"Some little time ago, in observing Jupiter and his satellites, I remarked certain emanations which appeared to have their origin in diffraction. I was very much puzzled to imagine in what way these phenomena could arise. My 4·2-inch Ross object-glass is simply perfect; my eye-pieces were carefully cleaned, and, so far as I could see by removing them, and looking up the tube at the Moon or Jupiter, the tube itself was free from any obstruction. Since, however, the general definition of the instrument was sensibly unimpaired, I took no further action in the matter, and let things take their course until about the middle of last month. I was observing the sun one morning at that period, when I removed the eye-piece for some reason, and happened to glance obliquely up the tube. To my astonishment I saw, brilliantly illuminated by the sun, a perfect *grating* of excessively fine spider-webs, spun vertically across the interior of the telescope, somewhat within the focus of the object-glass. A light, in more senses than one, suddenly broke in upon me, and I very speedily removed the offending lines. I had the pleasure, that same evening, of viewing the Jovian system shorn of all optical appendages. I am too ignorant of the Arachnida to be able to guess at the species which produces a web of such extraordinary tenuity; but it certainly must be an extremely minute one, not only on account of the excessive fineness of the filaments which it spins, but also in order that it should have found its way inside of a tube so thoroughly and carefully closed as (I should think) to prevent the existence of interstice or aperture whatever whereby an entry might be effected."

Spectrum of the Zodiacal Light.—In our last Summary we mentioned that Liais had found the spectrum of the zodiacal light to be a faint continuous one. Before that statement had appeared in these pages, news was received from the skilful Italian spectroscopist Respighi, to the effect that the spectrum of the zodiacal light consists in the main of the bright line forming the chief constituent of the spectrum of ordinary green aurora. "Formerly," he says, "I made spectroscopic observations on the zodiacal light, in the East Indies, but I could not see Angström's line, and I had obtained no result because I did not take the necessary precautions, protect-

ing the eye from external light whose brightness was sufficient to veil the line and the bright neighbouring zone." But his observations on the night following the great auroral display of February 4, serve, so far as they go, to prove the very reverse of what Respighi infers. He says: "Thinking that the aurora-borealis would re-appear on the next evening after the disappearance of twilight, I set myself to observe the sky, and I found it illuminated in all parts by a feeble light which produced the effect of a general phosphorescence. While waiting for marked phenomena, I directed the spectroscope provisionally towards the zodiacal light, then tolerably bright, and soon I could distinguish the green light and the neighbouring zone of light apparently continuous, and which embraced the space occupied by the lines of the auroral spectrum. Next turning the spectroscope on the faint light which illuminated the heavens, first towards the magnetic meridian, and then towards all azimuths and at all heights, I was surprised to find still the same spectrum, more or less marked, but everywhere as distinct as in the zodiacal light. Moreover Dr. D. Legge, one of the assistants at the observatory, distinctly saw this spectrum in all parts of the heavens. These observations were made towards seven or eight o'clock. Later, towards ten, I could not detect this spectrum in any part of the heavens. This fact, which confirms a similar observation made by Angström in March 1867, seems to me somewhat important, for it would tend to show the identity of the light of the aurora-borealis and the zodiacal light, and hence the probable identity of their origin." It seems to us, on the contrary, that Respighi's observation tends to show that the appearance of the auroral line when the spectroscope was turned upon the zodiacal light was due to auroral light in that direction, and not to the zodiacal light at all. It certainly was a suspicious circumstance that a certain bright line could be seen as distinctly in all parts of the sky as towards the zodiacal light; and one cannot understand why, under the circumstances, Respighi should judge this line to belong to the zodiacal light, since unquestionably any auroral phosphorescence must have extended over the zodiacal region as well as over the rest of the heavens.

But all doubts on the subject seem to be finally removed by Professor Piozzi Smyth's recent observations in Sicily. In the first place, it should be noticed that Prof. Smyth attended much more carefully to the structure of his instrument than any of his predecessors in zodiacal observation. He was also particularly careful to exclude extraneous light. He had an instrument so contrived that he could examine simultaneously the spectrum of any auroral light and the faint linear spectrum of an alcohol flame. Now with a narrow slit he obtained from the zodiacal light no spectrum at all, though the very same instrument had, with the same slit-opening, shown the chief auroral line even from very faint aurora. As the slit was widened no trace of a linear or band spectrum could be recognised, but with sufficient opening a faint continuous spectrum, exactly like that obtained from faint twilight, or indeed from the ordinary night-sky when there is no aurora. Here we have absolutely perfect proof of the fact that the zodiacal light gives the same spectrum as faint reflected sunlight. But Prof. Smyth's observation proves more than this. It shows that, under the very same circumstances, the zodiacal light gives no spectrum where the faintest

auroral light gives the characteristic green line spectrum; or, in other words, that spectroscopically dealt with, the slit being fine, the zodiacal light gives a background on which the faint auroral line (if aurora is present) may show itself as perfectly as though there were no zodiacal light at all. Hence Prof. Smyth's observation not only demonstrate the real nature of the zodiacal spectrum, but shows how the mistake of those arose who have supposed the zodiacal spectrum to be identical with that of the aurora.

Proper Motions of the Stars.—Dr. Huggins has been able to continue his researches into the proper motions of the stars in the direction of the line of sight. It will be remembered that the telescope he formerly used did not possess sufficient light-gathering power to deal with any star except Sirius. The instrument Dr. Huggins is now using has a light-gathering power four times as great. The first fruits of its employment in this line of research confirm in a very interesting manner the anticipations as well as the theories of Mr. Proctor. Dr. Huggins finds that certain stars are moving as if in systems or families, since they possess a common motion either of recess or approach. Among such instances may be mentioned one of a very remarkable kind. It may be remembered that Mr. Proctor, nearly three years ago, announced that the five stars β, γ, δ, ϵ, and ζ Ursx Majoris, as well as Alcor close by ζ, and the telescopic companion of ζ, are moving in a common direction; and at a lecture delivered in May, 1870, at the Royal Institution, Mr. Proctor expressed his conviction that whenever Dr. Huggins applied the spectroscopic method to these stars, he would find that they are either all receding or all approaching. Many, unaware of the evidence on which this conviction was based, considered so definite a prediction altogether unwise. It has, however, been amply confirmed by the event, since Dr. Huggins finds these five stars to be all receding at the rate of about thirty miles per second. On the other hand the star ζ, which Mr. Proctor had indicated as *not* belonging to the set, is found to have a spectrum differing in character from that common to the five stars, and though receding, has a different rate. The star α, also as marked by Mr. Proctor distinct from the rest, is found to have a totally different spectrum, and to be approaching. Thus the prediction referred to has been more than fulfilled; it has been found not merely that all the stars of the set are receding at the same rate, but that other stars excluded from the set are not moving in the same way, and are furthermore distinguished by spectral differences from the members of the drifting-star family.

Planets for the Quarter.—Saturn is the planet best placed for observation during the approaching quarter. He comes to apposition on July 9, at 11 h. 13 m. Jupiter will be in conjunction with the Sun on August 3, 4 h. 5 m. p.m., and is unfavourably situated for observation throughout the quarter. None of the other planets (except Mercury) will be well placed during the quarter. Mercury will be at his greatest eastern elongation on August 3rd, and at his greatest westerly elongation September 15.

August Meteors.—We remind our readers to look out for the famous August meteors on the nights of August 9, 10, and 11.

BOTANY AND VEGETABLE PHYSIOLOGY.

Dispersion of Seeds by the Wind.—A good paper on this subject has been contributed by M. A. Kerner, Director of the Botanic Garden of Innsbruck, to the "Zeit. des Deutsch. Alpen-vereins." He made a thorough inquiry into the flora of the glacier moraines, and the seeds found on the surface of the glaciers themselves, believing that these must indicate accurately the species whose seeds are dispersed by the agency of the wind. Of the former description he was able to identify, on five different moraines, 124 species of plants; and a careful examination of the substances gathered from the surface of the glacier showed seeds belonging to thirty-six species which could be recognised with certainty. The two lists agreed entirely in general character, and to a considerable extent, also specifically; belonging, with scarcely an exception, to plants found on the declivities and in the mountain valleys in the immediate vicinity of the glacier; scarcely in a single instance even to inhabitants of the more southern Alps. M. Kerner's conclusion is, that the distance to which seeds can be carried by the wind, even when provided with special apparatus for floating in the air, has generally been greatly over-estimated; and this is very much in accordance with the view advanced by Mr. Bentham in his anniversary address to the Linnæan Society of London in 1869. Along with the seeds M. Kerner found, on the surface of the glacier, more or less perfect remains of a number of insects belonging to the orders *Lepidoptera*, *Hymenoptera*, *Diptera* and *Coleoptera*, which, like the seeds, belonged almost exclusively to species abounding in the immediate neighbourhood of the glaciers.

The Fertilization of Coniferæ has been very carefully studied lately by Signor Delpino, who is now Professor in the State Forest School at Vallombrosa. He has been paying much attention to dichogamous flowers, and to the difference between those fertilised by the wind (anemophilous), or by insects (entomophilous), or by animals of whatever sort—zodiophilous, as he terms them. *Coniferæ*, as is well known, are anemophilous, that is, their fecundation is entrusted to the wind; their light and most abundant pollen is correlated to this, and the structure of the fertile inflorescence is such that the pollen reaches the very orifice of the ovule. In yew and cypress, and in other, if not all other genera of the sub-orders they represent, Delpino finds that, at the time when the ovule is ready for fecundation, a minute clear drop of liquid appears at the orifice of the ovule; grains of pollen falling upon this are retained, are incited by it to develop the pollen-tube into the liquid first, thence into the ovule, and the drop is then re-absorbed or dries up. Alph. de Candolle, in a recent number of the "Arch. des Sciences de la Bibl. universelle," calls attention to the fact that this droplet was known, as to its appearance, function, and re-absorption, to his late venerable townsman, Vaucher, and is described in his "Physiology of the Plants of Europe," published in 1841.

Action of Foreign Pollen on the Fruit of the Fertilised Plant.—"Silliman's American Journal," May, states that Maximowicz has collected in a Russian journal the observations and experiments on this subject, and recorded some observations of his own. He mutually crossed *Lillium davuricum* and *L. bulbiferum*. Now, these have been taken for one and the same by late

writers, but are really characterised, according to Maximowicz, by the form of their capsules and bulb-scales. In the single experiment the pistil of *L. bulbiferum* fertilised by the pollen of *L. davuricum*, set fruit, but failed to mature it. That of *L. davuricum*, fertilised by the pollen of *L. bulbiferum*, matured well; but, to the surprise of the observer, it formed the long capsule of *L. bulbiferum*, instead of the short one of the species. This is an important experiment, but it requires repetition.

The Phænogamous Plants of the United States east of the Mississippi, and the Vascular Cryptogamous Plants of North America north of Mexico.—This is the second edition, revised and corrected, and published now by Mr. B. Pickman Mann. It is, however, merely a reprint, with corrections and a few additions, of the catalogue known familiarly as "Mann's Exchange List," which has been of great service to all collectors of American plants. Since the running numbers have not been changed, it will be possible to use the old and new editions interchangeably. The "typographical and other errors," which have been found and corrected, are over 150, and Mr. Mann, in his preface, renews his brother's request that persons using the catalogue would send him notice of all errors discovered.

A Fungus-like Growth on the Leaves of Coleus Plants has been thoroughly investigated by Mr. H. J. Slack, F.G.S., who has read a paper upon the subject before the Royal Microscopical Society. In the first place, he says, a number of leaves were taken from coleus plants of various colours, and carefully examined in their natural state, both by transmitted and reflected light. It became apparent that every leaf, whatever its age or tint, exhibited chiefly, if not entirely on the under surfrce, a number of globular bodies of a beautiful yellow colour, highly translucent and refractive, most of them marked with a cross like that impressed upon the well-known cross-bun. These bodies differed in hue from any yellow of the leaf, and they were distributed pretty uniformly without any regard to the variegations of the leaf-colouring matter. From damaged specimens, it was obvious that they were the bodies alluded to by Mr. Howse, who in a recent paper imagined them of fungous origin. The colour of these bodies, when looking healthy, and well filled with their refractive matter, varied from rich topaz to a pale sherry tint, and they glittered like jewels when well lit up. Empty cells had a rude resemblance to a mushroom in form, with a stout stem and a round head marked with the cross, but the texture did not look in the least fungoid, nor could any mycelium be discerned in or on the leaves.—*Vide* "Monthly Microscopical Journal," May.

Death of M. de Brebisson.—Those who are familiar with the large amount of work done by this gentleman will regret his death, which took place on April 26, in his seventy-fourth year.

Structure of the Diatomaceous Frustule.—Professor H. L. Smith gives his views on this subject in the April number of the "Lens." He believes that all the frustules are "siliceous boxes," with either one portion (the cover) slipping over the other, as in *Pinnularia*, or with edges simply opposed, as in *Fragilaria*. If we take a frustule of *Melosira*, it may be compared to a pill-box—one portion slipping on to the other. The great majority of diatoms are thus constituted. It is perfectly evident that, in the case of the formation of a new valve, in the processes of self-division, this new

part, which slips out from the older, must be somewhat smaller. Inside of the box is a membrane, inclosing the internal coloured or colourless substance, imbedded in which may always be seen, at least in the larger forms, a distinct nucleus, sometimes two, and sometimes a "germinal dot," with numerous fine threads radiating from the nucleus, or the germinal dot. As the frustule increases in width, one portion slips out from the other, and sometimes successive additions of siliceous matter are made to the edges of the box, somewhat analogous to the successive additions to the edge of the shell of a mollusc. When the widening of the frustule has reached a certain extent, the lining membrane, at the places which would be exposed if the two portions were wholly to slip apart, infolds. He has reason to believe that, previous to this, a double membrane of extreme tenuity has been formed, commencing its growth *at* the nucleus (which itself is divided), and extended to the margins of the cell, which is thus divided into two nearly equal parts; for, as soon as this infolding commences (perhaps now accelerated by the admission of water), the line of division can be seen progressing steadily inwards by the parting of this thin double membrane, so that in fifteen or twenty minutes the fissure is complete. He has, in very large *Pinnulariæ*, witnessed the whole phenomenon, from its inception up to the final self-division. While the *fissure* occurs in the short period of time he has named, to complete the *self-division* requires about six days.

Is there Alternation of Generations in Fungi.—Mr. M. C. Cook, M.A., believes that it is questionable whether this phenomena occurs in fungi, as Professor Œrsted alleges. He thinks it takes place in the same plant, as in the case of *Bunt*; but he feels great difficulty in believing in this process, where the generations were passed in different plants, until confirmed by other observers. If the spores of *Æcidium Berberidis* were taken from the barberry and sown upon young wheat plants, and all these plants became infected with corn mildew (*Puccinia graminis*), to which wheat is but too prone, it certainly seemed premature to say that the spores of the *Æcidium* caused the *Puccinia* to be developed as a second generation; whereas it is much more probable that the germs of the mildew already lay dormant in the wheat, and, at most, the sowing and growing of the *Æcidium* spores only stimulated the mildew to a more rapid development.

Altering the Name of a Bog-moss.—Dr. Braithwaite is sufficiently conscientious in giving a new name to a species. In his last paper in the "Monthly Misroscopical Journal," June, he gives the following observations on this subject. In Phænogamic Botany, Entomology, and other departments of natural history, the adoption of the first name by which a species has been described (dating from the establishment of the binomial nomenclature by Linnæus) is considered imperative; yet the synonymy of mosses is wofully confused, for Hedwig and others gave a new specific name as often as they changed the genus—a rule not sanctioned by the best authorities. Others may object with greater reason that the brief discriptions of the older authors are not sufficient to identify the species with certainty, yet it must be remembered that the actual specimens of very many of them are in existence, and their examination by a competent authority in most cases settles the question. Prof. Lindberg, who has worked so indefatigably at this unattractive department of botanical literature, has shown, in his "Rev.

Crit. Ic. Musc. Fl. Dan.," that this species is in the St. Petersburg herbarium named *tenellum* in Ehrhart's own handwriting; this, however, without description, might not be allowed to stand, but the same species received the same name from Persoon, as proved by a specimen from him, preserved in Swartz's herbarium; and a description is given by Bridel in his "Mantissa Musc." (1819), the leaves indeed being described as recurved at the point, which might perhaps refer to them in a dry state. Bridel also admits *S. molluscum* into the Bryol. Univ., but he only copied the description of Bruch (1825), without having seen a specimen. Dr. Braithwaite, therefore, has no hesitation in adopting the name first given to the species.

The Breathing Pores of Leaves.—A good popular paper on this subject is that which Prof. T. D. Biscoe read before the Troy Scientific Association, and published in the "American Naturalist," March, 1872. If, he says, the outer layer or skin be stripped from the surface of the green-coloured parts of plants, and examined under a low power of the microscope, the stomata, or breathing pores, will appear as green specks in the otherwise colourless membrane. Their object is to open and close communication between the intercellular space always existing between the individual cells and the outer atmosphere. The sausage-shaped cells constituting the essential part of the organ are called the pore cells. They have the power of separating from each other in the middle, thus opening a free way for the air to the interior tissues; or in certain conditions of light and moisture they approach each other so as to narrow or entirely close the slit between them. They are filled with protoplasm, chorophyl and starch granules, while all other cells of the outer surface are filled only with air and water. Apparently with the object of placing these pore cells as free as possible from all constraint or pressure, so that they may correspond sensitively to all the changes in the atmosphere, they are at times situated on a level with the epidermis cells, sometimes raised above, at others sunk beneath this level. If the epidermis cell-walls are thin and flexible, the stomata will generally be found in the same surface with them; but when the epidermis walls are thick and stiff, the stomata will generally be found sunk deep under the surface, or raised above it, or surrounded by a ring of smaller cells with thinner walls than the remaining epidermis cells. Immediately under the stomata are empty spaces, of irregular form and varying size, called breathing-rooms. They are in connection with, and form a part of the intercellular space which ramifies through the entire structure of most tissues. It is an interesting question in what way the stomata have been formed. Were the pore cells at first a pair of ordinary cells, which have gradually changed their form and contents until endowed with all the peculiar properties of their natural state? Or were they always existent in their peculiarities, only smaller as the leaf was younger? Or, have they grown out of a single cell by the process of subdivision and after-growth? Do they belong to the epidermis, or to the chlorophyl-bearing tissues beneath?

CHEMISTRY.

The Chemical Society's Faraday Lecture.—On Thursday, May 30, Professor Cannizzaro, of Palermo, delivered the "Faraday Lecture" before a large audience, including a number of ladies. The Lecture Theatre of the Royal Institution had been kindly lent to the Chemical Society for this purpose, and the learned Professor's discourse was entitled "Considérations sur quelques Points de l'Enseignement Théorique de la Chemie." On the following Friday a dinner was given to the Professor, at which about 150 were present, including the Italian ambassador and the Right Hon. the Chancellor of the Exchequer. We think it is a pity that some arrangement is not made by which the lecturer could address his audience in English, for we are certain that very few of them apprehend French sufficiently well to take in even the substance of the lecture.

Chemical Analysis of the Meteoric Rain in Sicily of March 9, 10, *and* 11. —M. O. Silvestri has an important memoir on this subject in the "Comptes Rendus," April 9, 1872. This memoir contains the results of the researches made on rain-water along with which fell a kind of sand; the water, having been filtered, was found to be colourless and free from smell, but exhibited a saline taste; it was neutral to test-paper; hardness, 17·5 degrees (ordinary rain-water, 1 degree). By long-continued boiling, it gave off $19\frac{1}{2}$ c.c. of gas, consisting of: nitrogen, 83·959 per cent.; oxygen, 13·070; CO_2, 2·971. On being evaporated to dryness, this water was found to contain, in 1,000 parts: bicarbonates of lime, 0·129; of magnesia, 0·035; of iron, traces; sulphate of lime, 0·041; chloride of potassium, traces; sulphate of soda, 0·009; chloride of sodium, trace; organic matter, 0·063. The sand, very finely pulverised and dust-like, has a sp. gr. $-2·5258$, and contains, in 100 parts: clay, 75·08; carbonate of lime, 11·65; organic matter, 13·10.

Dr. Hoffmann on Phosphuretted Hydrogen.—On Dr. Hoffmann's recent visit to this country, he went to the Chemical Society as a matter of course. On April 18 he there experimentally exhibited the formation of phosphuretted hydrogen by the action of water on iodide of phosphonium, and the decomposition of iodide of ethyl-phosphonium by water, liberating the ethyl-phosphine, E_2HN, which has the characteristic odour of the phosphorus bases. He also explained Baeyer's method of preparing iodide of phosphonium on the large scale by the action of water on iodide of phosphorus, stating that it was necessary to employ a large excess of phosphorus, three or four times the theoretical, since much of it is converted into the amorphous state, and thereby rendered inactive.

Chloride of Sodium in Liebig's Extract of Meat.—In the "Annalen der Chimie" for May, Baron Justus von Liebig has written a paper with the view to refute the allegation made by a Dr. Godefroy, who appears to have published, in an Austrian scientific paper, a statement to the effect that Liebig's extract of meat should contain 2 per cent. of chloride of sodium, purposely added as a fraud. The author refers Dr. Godefroy to his (Liebig's) essays, published in the "Annalen" twenty years ago, "On the Constituents of the Fluids contained in Meat," and emphatically denies that at Fray Bentos, where the extract of meat is made, any common salt is added to it. Chloride of potassium is largely contained in the extract.

Determining Carbonic Acid in Sea-water.—Professor Himly, at the meeting of the Chemical Society, April 18, after pointing out the difficulties which beset the determination of carbonic acid in sea-water—Jacobsen having shown that the whole of the gas present is not given off by boiling, either *in vacuo* or under the ordinary pressure at 100°, even when a current of air is passed through the liquid—said, however, that the whole of the carbonic acid could be readily estimated by adding baryta water or barium nitrate, ammonium nitrate, and ammonia, to a measured quantity of the sea-water, thus obtaining the whole of the carbonic acid in the precipitate. After the supernatant liquid had been removed, the carbonic acid might be estimated in the precipitate. In order to collect sea-water for the determination of the carbonic acid at great depths, and consequently under great pressures, it was necessary to sink a cylinder open at both ends to the place where the water was to be collected, and then to securely close it there. The apparatus for that purpose, closed by valves, had been found to be very defective; but he had employed one which answered admirably, consisting of a cylinder furnished with a large stop-cock at each end. When this cylinder had been sunk to the required depth, the stop-cocks were closed by powerful springs released at the proper moment by means of an electro-magnet set in action in the usual way. (See also "Chemical News.")

A New Organic Pigment has been obtained from a spot above the eyes of the moor-cock. It is called *Tetronerythrin*, and has been described by Dr. Wurm in "Poggendorff's Annalen." It seems ["Chemical News"] that a statement was made in the "Wiener Jagdzeitung" to the effect that the red warty spot met with above the eyes of the mountain-cock and moor-cock (*Tetrao tetrix*), when rubbed with a white handkerchief, imparted thereto a beautiful red colour. The author was inclined to disbelieve this, and accordingly made some microscopical and microchemical researches on this subject, the result being that he discovered a pigment which he terms *Tetronerythrin* (from *Tetraon* and *erythros*, mountain-cock red). A very small quantity of this pigment, which is soluble in chloroform, was sent by the author to Dr. J. von Liebig, who states that it is a peculiar substance which has nothing in common with the colouring matter of the blood; it is soluble in ether and sulphide of carbon, not soluble in cold caustic alkaline solutions, and soluble in hot nitric acid, but decomposed simultaneously, leaving a waxy residue.

How to Know Fruit-wine from Grape-wine.—According to Dr. F. Vorwerk ["Neues Jahrbuch für Pharmacie"], the phosphoric acid present in genuine grape-wine is combined with magnesia, while in fruit-wines it is present in combination with lime. The simple addition, therefore, of ammonia (1 part to 9 parts of wine), will produce in genuine wine, after twelve hours' standing, the well-known precipitate of ammonio-phosphate of magnesia.

Dr. Letheby and Dr. Frankland on Water-analysis.—The editor of the "Chemical News" has quite recently given a leading article on this important subject. Referring to the controversy which has come up upon the matter, he states that if it had arisen in France there would have been a commission of the Academy to report on it. In this country we manage things differently, and resolve questions of this description after our own

fashion, and we have to collect the testimony of competent scientific men who live and work in isolation. Mr. Way, who was formerly on the Rivers' Commission, has published his judgment on this question in a report on the analysis of a sample of water. It is to the effect that he uses and has confidence in the rival process of Wanklyn and his colleagues, which, it is admitted, gives results in opposition to Dr. Frankland's. Dr. Angus Smith, who, as our readers possibly know, has been for some time engaged in a very important investigation of the organic matter existing in the atmosphere, has also given in his adhesion to the other process, which he employs in his researches. The late Dr. W. A. Miller rejected Dr. Frankland's process and employed Mr. Wanklyn's in his later investigations, undertaken for the medical department of the Privy Council. Dr. Voelcker, chemist to the Royal Agricultural Society, rejects Dr. Frankland's process and adopts the other. Dr. Letheby has often expressed a like opinion. Indeed, says the writer in conclusion, we scarcely know a single chemist of reputation who approves of Dr. Frankland's water-analysis ! !

The asserted Alkalinity of Carbonate of Lime.—Mr. William Skey, Government Analyst, New Zealand, re-asserts the alkalinity of the above. He says, in a paper of his which appeared in the second volume of the "Transactions of the Wellington Philosophical Society, that he asserted the alkalinity of carbonate of lime, but the correctness of this assertion having been disputed by Mr. Charles R. C. Tichborne, F.C.S., M.R.I.A., &c., of the Laboratory of the Apothecaries' Hall, Ireland, in a communication to the editor of the "Chemical News" (vol. xxii. p. 150), he has re-investigated this subject and extended his researches upon it, by which he has arrived at results corroborative of the correctness of his statement, and which show, besides, that a large number of salts hitherto maintained to be neutral, or respecting which nothing has been affirmed, are in reality alkaline. This is important; for it may be regarded as conclusive so far as Mr. Skey's researches are concerned.—*Vide* "Chemical News," March 28.

The new Hydrocarbon : Abietene.—Mr. W. Wenzell, who writes in the "American Journal of Pharmacy," March, 1872, says that this hydrocarbon is the product of distillation of the terebinthinate exudation of a coniferous tree indigenous to California, viz., the *Pinus sabiniana*, a tree met with in the dry sides of the foot-hills of the Sierra Nevada mountains, and locally known as the nut-pine or digger pine, owing to the edible quality of its fruit. A gum resin, or rather balsam, is obtained from this tree by incisions made in its wood, and the balsam submitted to distillation almost immediately after having been collected, owing to the great volatility of the hydrocarbon (or essential oil, because abietene really stands in the same relation to the balsam alluded to as oil of turpentine stands to the exudation derived from other *Pinus* species). The crude oil, as usually met with for sale at San Francisco, is a colourless limpid fluid, requiring only to be redistilled to obtain it quite pure. The commercial article is used under different names—abietine, erasine, theoline, &c.—for the removal of grease and paint from clothing and woven fabrics, and likewise as an efficient substitute for petroleum-benzine. The ultimate composition of abietene is not stated, but the author points out at some length the differences existing between abietene and terebene (oil of turpentine).

Influence of Pressure in producing Chemical Change.—An important paper on this subject has been read before the Chemical Society, May, 16, by Mr. H. T. Brown. In his investigation the author found that during the alcoholic fermentation of grape juice or malt wort, besides carbonic anhydride, that nitrogen, hydrogen, a hydrocarbon of the paraffin group, and sometimes nitric oxide, are evolved; moreover, the proportion of the gases unabsorbed by potassium hydrate is largely increased when the operation is carried on under diminished pressure. At the ordinary pressure by far the larger proportion of these gases is nitrogen (70 to 90 per cent), but under diminished pressure, 400 to 459 m.m., the hydrogen preponderates (60 to 90 per cent). Nitrogen, however, does not occur when the solutions contain no albumenoids, even if ammonium salts are present in considerable quantity. The increase of the proportion of hydrogen, resulting from diminution of the pressure, is accompanied by formation of a comparatively large amount of acetic acid and aldehyde, so that it would seem that water is decomposed during the alcoholic fermentation, and that this result is facilitated by the diminution of the pressure. The presence of nitric oxide in the evolved gases was found to be due to the reduction of nitrates originally present in the solutions.

Fearful Adulteration of Whisky in Ireland.—At a recent meeting of the Chemico-Agricultural Society at Belfast, under the presidency of Dr. Knox, late Poor Law Inspector, the subject of whisky adulteration was brought under consideration by Dr. Hodges, Professor in Queen's College, Belfast, who exhibited a specimen of that liquid brought to him by two men who had been physically incapacitated by drinking a small portion of it in a public-house. He found, on analysis, that it contained a large amount of naphtha. He had also discovered that ingredients of even a more deleterious character were used in the process of adulteration—mixtures containing sulphate of copper (blue stone), cayenne pepper, sulphuric acid (vitriol), and a little spirits of wine. One specimen submitted to Dr. Hodges by a number of provision-cutters and curers, was composed of naphtha and a slight colouring of whisky. The men who had imbibed a small quantity of it were affected with serious symptoms; and this, said Dr. Hodges, was a fair specimen of the drink sold in low-class public-houses. The trade in this noxious compound is carried on with impunity, no local authority in Belfast or in the Province of Ulster caring to interfere with it.

A Substitute for Soda in washing Linen.—This, which appears to be extensively used abroad, will, we doubt not, prove of great service if introduced into the English laundry. It is described by Dr. Quesneville, in the *Moniteur scientifique Quesneville* (March). The very common use, says Dr. Quesneville, especially in England, of soda for washing linen is very injurious to the tissue, and moreover has the effect of yellowing it in the long run. The author states that in Germany and Belgium the following mixture is now extensively and beneficially used :—2 lbs. of soap are dissolved in 25 litres (5·5 gallons) of water, as hot as the hand can bear it; there are next added to this fluid three large-sized tablespoonfuls of liquid ammonia and one spoonful of best oil of turpentine. These fluids are incorporated rapidly by means of beating the soap-suds and other fluids with a small birch-broom. The linen, &c., is then put into this liquid and soaked for three

hours, care being taken to cover the washing-tub with a closely-fitting wooden lid; by this means the l'uen is readily cleansed, requires hardly any rubbing, and less brushing, and there is a saving also of time and fuel. Ammonia does not affect the linen nor woollen goods, and is largely used as washing-liquor in the North of England, of course along with much water, as above indicated.

GEOLOGY AND PALÆONTOLOGY.

Fossil Turtles.—At the meeting of the American Philosophical Society, March 1, 1872, Professor Cope read a paper "On *Protostega*," a genus of extinct Testudinata. A detailed account of the osteology of *P. gigas* from the cretaceous beds was given, by which it appeared that the genus had separate ribs, as in *Sphargis*, and that the carapace was formed by large radiating plates of bone in the skin. Two other species were described—*P. tuberosus* and *P. neptunus*. The latter, the largest known marine turtle, from New Jersey; the former, from the cretaceous of Mississippi, had been referred by Leidy to the Mosasauroids.

Mr. Waterhouse Hawkins' efforts in New York have, we regret to learn, been completely overthrown by an ignorant manager of the Central Park Museum. In answer to an inquiry made of him, Mr. Hawkins says that all he had done during twenty-one months to restore the skeletons of the extinct animals of America (of the Hadrosaurus, and the other gigantic animal, which was thirty-nine feet long), was destroyed by order of Mr. Henry Hilton, on May 3, with sledge-hammer, and carted away to Mount St. Vincent, where the remains were buried several feet below the surface. The preparatory sketches of other animals, including a mammoth and a mammoth and a mastodon, and the moulds and sketch-models, were destroyed. Mr. Hilton did this, said Mr. Hawkins, out of ignorance, just as he had a coat of white paint put on the skeleton of a whale which Mr. Peter Cooper had presented to the Museum, and just as he had a bronze statue painted white. Mr. Hilton told the celebrated naturalist who had come from England to undertake the work that he should not bother himself with "dead animals;" that there was plenty to do among the living. This illustrates the policy of having such men as Hilton at the head of one of the most important departments of the City government. When the skeletons were dug up again, by order of Colonel Stebbins, they were found broken in thousands of pieces. Prof. Henry, of the Smithsonian Institution, when he heard of this piece of barbarism, would not believe it. "Why," he exclaimed, "I would have paid them a good price for it." Mr. Hilton, however, preferred to destroy the work of the naturalist, which had cost the city at least 12,000 dollars.

Flint Arrow-heads.—We beg to recommend our readers to an excellent article on these weapons, which appears in the "American Naturalist" for April. It is certainly the ablest paper on this subject that we have seen, and it is abundantly illustrated.

New Species of Cretaceous Birds.—A very able paper on this subject appears in "Silliman's American Journal" for May, by Mr. O. C. Marsh. The few remains of birds hitherto described from the cretaceous deposits of

that country, although of much interest, all pertained to comparatively small species, and belonged, apparently, to families still existing. It is fortunate, therefore, that the existence of a fossil bird, so large and remarkable as the one (*Hesperornis regalis*) that forms the subject of the present description, should first be made known by the discovery of such important parts of a skeleton as to afford ample material for the determination of its affinities. This interesting discovery has already been announced in "Silliman's Journal," and the name, *Hesperornis regalis*, proposed by the writer for the species thus represented. The present paper is preliminary to a full description, with illustrations, now in course of preparation. The other species briefly described in this article are likewise of interest, as they add some new forms to the limited avian fauna heretofore found in the cretaceous beds of the Atlantic coast.

Changes of Climate during the Glacial Period.—We observe that Mr. Geikie has communicated his seventh and last paper on this subject to the "Geological Magazine" for June. The series are of very great importance, though of course it is utterly impossible for us to give an abstract of the several papers it includes. However, for our readers' convenience of reference, we give the following, which is the order of appearance of the earlier portions of Mr. Geikie's paper "On Changes of Climate during the Glacial Epoch."

First Paper:	"Geol.	Mag."	vol.	viii.	Dec.	1871,	p. 545.
Second „	„	„	„	ix.	Jan.	1872,	p. 23.
Third „	„	„	„	„	Feb.	„	p. 61.
Fourth „	„	„	„	„	March	„	p. 105.
Fifth „	„	„	„	„	April	„	p. 164.
Sixth „	„	„	„	„	May	„	p. 215.
Seventh „	„	„	„	„	June	„	p. 254.

The last Eruption of Vesuvius.—We merely record the fact here that there has been a severe eruption since our last issue. Mr. G. Poulett Scrope has given an account of the eruption in the "Geological Magazine" for June. However, until Professor Palmieri gives us his version of the tale, no other can have any very great value. It is not a little amusing to note, as is done by Mr. Scrope, F.R.S., how the peasantry have considered the Professor's bravery:—"Signor Palmieri, who watched throughout with creditable constancy the progress of the eruption, from his observatory on the Crocelle, appears by so doing to have gained a character for almost superhuman heroism among the frightened population of Naples and its environs. The philosopher must have been much amused at the fervour of his extravagant admirers, who raised him almost to the level of their adored St. Januarius; knowing as he well did, of course, the very small amount of danger that he incurred while he remained at his post, under a substantial roof, above the possible reach of any lava-stream, in a building founded on a portion of old Somma, which has certainly never been seriously disturbed for the last 1,800 years. He, better than any one, knows that the phenomena of the late eruption were by no means so exceptional as our newspaper correspondents would persuade us, but of the ordinary type of moderate Vesuvian paroxysms, such as the mountain has exhibited perhaps a dozen times within

the last hundred years. That, indeed, is the judgment he is said to have passed upon it.

Eocene Fossil Wood.—This subject receives the attention of Professor T. Dyer, and, as is customary with Mr. Dyer's labours, it has been gone into fully and exhaustively. He explains fully the nature of so-called *tylose*, a subject upon which we have not been very clear before. He says that many instances of tylose are now known amongst recent plants, and have been repeatedly made the subject of investigation by foreign writers. Malpighi, indeed, in his "Anatome Plantarum," gives a very fair representation of them in the oak, remarking, "fistulæ frequenter *pulmonares* quasi *vesiculas* trachearum substantia excitatas continent." Without going into the literature of the subject, which is considerable, it is sufficient to state that the investigations of an anonymous writer in the "Botan. Zeit." for 1845, confirmed by Mohl and Reess ("Bot. Zeit." 1868), appear to leave little doubt that the "Thyllen," as the first-mentioned writer named them, are hernioid protrusions into the vessel from adjacent cells. In the words of Reess, "each young thylle makes its appearance as a bulging of a wood-parenchymatous or medullary-ray cell forced through a pore in the vessels." This process would be inconceivable in the case of the prosenchymatous cells; but parenchymatous cells, which surround the ducts, and those which form the medullary rays, do not undergo the same amount of speedy induration.

Sauropus Unguifer, a new Carboniferous Batrachian, has been just described by Dr. J. W. Dawson, F.R.S., of Montreal. He states that the principal specimens are several large slabs of brownish sandstone, bearing series of footprints in relief. Of the largest and most distinct series 40 to 50 footprints have been preserved, and are arranged in two rows, about $5\frac{1}{2}$ inches apart. They were probably produced by a large Labyrinthodont Batrachian walking on a muddy shore, near the edge of the water, and are not very dissimilar from those described by Sir C. Lyell as found by Dr. King in the carboniferous beds of Pennsylvania. They also closely resemble, in size and form, the footprints found by Mr. R. Brown, F.G.S., in the coal-field of Sydney, Cape Breton, and described by Dr. Dawson in the second edition of "Acadian Geology," p. 358, under the name of *Sauropus Sydnensis*, and still more closely those found by Mr. Jones, F.L.S., at Parrsborough, N.S., and noticed in the same work. With these they may, in the meantime, be included in the provisional genus *Sauropus*.

The dimensions of the footprints are as follows:—

Hind foot, breadth	2·71 inches.
„ „ length	4·24 „
Fore foot, breadth	2 63 „
„ „ length	2·77 „
Length of stride	11·53 „
Average distance between the rows of footprints made by right and left feet	5·48 „

These measurements correspond very nearly with those of his *Sauropus Sydnensis* above referred to. He has given it the name of *S. unguifer*.

Fossil Plants from Queensland.—In the course of the discussion at a recent meeting of the Geological Society, upon a paper by Mr. R. Daintree, Mr.

Carruthers stated that he had examined the vegetable remains brought over by the author, which were of great importance. Some of those from the Devonian rocks appeared to be identical with species found in North America. From the remains of one of these, which he could not separate from one described by Dr. Dawson, *Leptophlœma rhombicum*, he had been able to reconstruct it in its entirety, of which he exhibited a drawing. The plant was lycopodiaceous, and its remains served to show that erroneous conclusions had been drawn as to the characters presented by the North American specimens, which had been regarded as having a *Sternbergia*-pith. There were specimens also of *Cyclostigma*, of the stipes of ferns, and of a doubtful calamite. With regard to the supposed *Glossopteris*- and *Tæniopteris*-epochs, which by some had been regarded the one as Palæozoic and the other as Mesozoic, he was not convinced that they could be distinctly separated, but thought rather that they might both belong to different portions of one great period. Systematically the two forms might be very closely related, the venation of the fronds on which the genera are founded occurring in two forms, which by Linnæus had been included in one genus, *Acrostichum*. He thought that neither was of a date earlier than Permian.

Death of Dr. Auguste Krantz.—We regret to announce the death of this distinguished collector of rocks, fossils, and minerals, which took place at Berlin on the 6th of April last. The "Geological Magazine" says of him that he represents one of the longest established and most able members of that rare class, a scientific merchant in rocks, fossils, and minerals—one, who not only knew accurately the commercial value of his collections, but was intimately acquainted with the scientific worth of every specimen which passed through his hands. Indeed, there are few museums which have not been enriched from his cosmopolitan repository. He leaves an immense and valuable collection both of minerals and fossils, the result of the labours of a long life devoted to these pursuits. Dr. Krantz was in his sixty-second year. We believe it is the intention of Madame Krantz to carry on her husband's business, with which she is well acquainted.

Amount of Coal in Austria and Hungary.—A very valuable and trustworthy report is that of Herr F. Foeterle, which has been recently published. It is accompanied by a large map, a glance at which will convince every one of the scanty distribution of coal over the enormous surface of the Austro-Hungarian dominions, and that most of it belongs to the western and the central districts. *a. True Coal-measures Coal* is found in Bohemia, in Moravia, and Austrian Silesia, in the Alps and in the Hungarian dominions. *b. Trias and Lias Coal* in the Alps, in Hungary and in the Banat. *c. Cretaceous Coal* in Moravia, in the Alps, and in Hungary. *d. Eocene Coal* (sometimes still showing the structure of the wood, then called Lignite, but generally a good black coal, which, when burnt, cakes, and is excellent for gas manufacture) is chiefly found in the *Alps*, where it is embedded in Cosina beds, below the Nummulite Limestone; Carpano near Albona, the large coal-basin of the Marburg district, Sotzka, Eibiswald. The coal of Häring, in Tyrol, belongs to a higher horizon of the Eocene, as does also the coal of Monte Promina and of Sebenico in Dalmatia. The coal of Gran, in Hungary, is also of Eocene age. *e. Neogene Coal* forms large basins in Moravia, Bohemia, Galicia, Bucovina, and in the north and south zones of

the Alps and in Hungary. A glance at the accompanying map of the distribution of fossil fuel in Austria shows at once how insignificant is the extent of her coal-basins in comparison with the coal-formations of England, North America, or even Prussia.

England has	8,960	square	miles	of coal.
North America	100,528	„	„	„
Province of Silesia in Prussia .	1,280	„	„	„
Austria (as near as possible) . .	1,200	„	„	„

The whole produce of coal of all formations in Austria and Hungary amounted during 1868, in round figures, to 6,300,000 tons.

Phaneropleuron and Uronemus.—Professor Traquair, M.D., writes to the "Geological Magazine" to say he has now satisfactorily determined that his fossil fish *Phaneropleuron elegans* is identical with the *Uronemus lobatus* of Agassiz. Of course it is well that Professor Traquair has made this discovery, but it would have been better had he taken more time at first to inquire into the facts of the case ere he gave a new name to the fish.

MECHANICAL SCIENCE.

Paper Armour.—Colonel Muratori, at present in this country, has been endeavouring to introduce paper as a material for resisting bullets, and even projectiles of greater weight. A cuirass which he has invented, made of this material, and weighing no more than the ordinary metal cuirass, is said to possess a much greater power of resistance. Experiments on this material were made at Chalons in 1868, under the direction of the late Emperor of the French. The war stopped the experiments, and Colonel Muratori is now seeking to have them resumed in this country.

Torpedo Warfare.—Mr. C. W. Merrifield, F.R.S., has suggested, at the Institute of Naval Architects, that structural means of resisting torpedo attacks should be provided in armour-plated vessels. Of possible means of meeting torpedoes, he thinks that a rope or wire netting outrigged at a distance of 6 or 8 ft. from the ship's skin would afford the best protection in cases where a line of torpedoes is known to exist. But such a netting offers so great an impediment to speed, that it would be impracticable to employ it when the object is to cruise at a risk of meeting torpedoes. Hence he is led to suggest the following device as better than armour-plating the ship's bottom. Let the ship have three skins, each divided into cellular spaces of moderate size, the middle skin representing what is now the outer skin of ordinary double-plated ships. Each cell between the middle and inner skin to have an airtight manhole by which access can be gained to it from the interior of the ship, and a stopcock and union collar in its upper corner. The space between the middle and outer skins is also to be divided into cells by frames breaking joint with those between the middle and inner skins. Water is to be freely admitted to the cells between the middle and outer skins, so that in fact the bottom of the ship would have a kind of water-casing surrounding the middle skin. The middle skin

is to be deliberately weakened near the bottom of each inner cell. When a torpedo explodes against the outer skin, it is expected that the shock will break through the middle skin at its weak points. Then the outer skin will be driven in, forcing the water into the cells between the middle and inner skins against the cushion of air contained in them. The work so expended will, it is hoped, save the inner skin from injury, except with very powerful torpedoes. After the explosion the inner cells may be cleared of water by attaching hose to the union joints and forcing in air. Nothing but actual experiment can decide on the value of such a plan, but it is believed to be the first suggestion yet made for providing structural means of resisting torpedo attack.

New Technical Journal.—Mr. E. J. Reed, C.B., the late Chief Constructor of the Navy, has started a new quarterly magazine dealing with subjects relating to naval architecture and marine engineering, which bids fair to render very great service, not only to those professionally engaged in the construction of ships and engines, but to the much larger circle of readers requiring information on such subjects of a reliable character. The articles in the first number of the new magazine are many of them by writers of eminent experience and knowledge, and range over a field including both special technical subjects, and others of more general interest. The articles on the stability of ironclads, the structure of iron ships, and the stowage of merchant ships, are of the former character. Those on the proposed Naval University, on naval tactics, and on the necessity of forming a naval staff, with the very severe review of the criticisms on the navy contained in Mr. Hawksley's presidental address, at the Institute of Civil Engineers, will be of interest to a very wide circle of readers.

H.M.S. Thunderer.—This powerful ironclad, a sister vessel to the *Devastation*, has recently been launched at Chatham. She is one of the mastless ironclads designed by Mr. Reed in 1869. The armour generally is 12 in. thick, but 14 in. in the neighbourhood of the port-holes. On the sides of the breastwork, in parts where a shot penetrating would do no harm to the machinery of the turrets, the armour is reduced to 10 ins., and it is also reduced to 10 ins. in the lower strake on the sides of the hull. The vessel has a sharp spur for ramming, and is short and handy. She is driven by engines guaranteed to give 5,600 indicated horses power, and will be armed with four 35-ton guns.

Wind Pressure on Inclined Planes.—Mr. Wenham and Mr. Browning have carried out some new experiments with a very delicate instrument on the pressure of a current of air on inclined planes. The results have been communicated to the Æronautical Society, and will be of interest not only to those who are studying the mechanism of flight, but also to engineers who have often to calculate the effect of wind pressure on their structures.

The Westinghouse Air-break.—This is a form of continuous break, or break applicable simultaneously to all the carriages of a railway train, which has been in use for about three years in America, and is now being tried in this country. An air-compressing pump is fixed on the locomotive engine, delivering the air into a reservoir under the foot-plate. The speed of the pump is self-adjusting, the valves being so set that it just keeps moving against the full pressure in the reservoir, and when the pressure in the

reservoir falls it immediately accelerates in speed. Duplicate lines of $\frac{3}{4}$ in. iron gas-piping extend beneath the train, with ingenious indian rubber connections between the carriages. The breaks can be worked by sending the air through either line of pipes, so that if one fails the other remains serviceable, and further with two lines of pipes the carriages may be turned end for end, if necessary. The pipes not being in the centre line of the carriage, this could not be effected unless two symmetrical lines of piping were provided. The breaks are actuated by the air pressing on a piston in an air cylinder placed under each carriage. Hence the breaks can be instantly put in action by turning a cock and admitting air from the air-reservoir to the piping. The pressure of air in the reservoir is 60 to 70 lbs. per square inch, but it is usually regulated not to exceed 10 to 30 lbs. in the air-cylinders. The details of this break, which are most ingenious and most carefully worked out, cannot be understood without drawings, and for these we must refer the reader to "Engineering" for May 24, where the invention is very fully described.

MEDICAL SCIENCE.

The Physiological Action of Tobacco has been very carefully studied by Herren Vogl and Eulenberg. They investigated the physiological action of (1) those bases volatile below 160° and (2) of those volatile between 160° and 250°. Both portions act like nicotine, producing contraction of the pupil, difficult respiration, general convulsions and death. They act more quickly by the stomach than when sub-cutaneously injected, but even then are not as prompt as nicotine. On post-mortem examination, the lungs and air-passages were found to be highly congested. They think that the disagreeable symptoms produced in the incipient smoker, and the chronic affections which excessive smoking produces, as well as the poisonous effects of tobacco-juice when swallowed, are due to the pyridine and picoline bases, and not to nicotine. They explain the fact that stronger tobacco can be smoked in cigars than in a pipe, by finding that more of the volatile bases are present in the smoke of a pipe; more especially of the very volatile and stupefying pyridine; while in a cigar, little pyridine and much collidine are formed. The authors compared this action with that of the bases obtained from other plants used for smoking; with those from dandelion, willow-wood and stramonium, and with pure picoline from Boghead coal. The action was entirely similar, but, with the exception of the willow wood bases, they produced no contraction of the pupil. Picoline in vapour is extremely poisonous, producing great irritation of the air-passages, convulsions and death. From these results the authors believe that the different effects of smoking opium are due simply to a difference in the proportion of the bases produced by its combustion.—*Arch. Pharm.* II. cxlvii. 130.

The Supposed Syphilis Corpuscles.—The "Monthly Microscopical Journal" of April, quoting from the "Allg. Wiener Med. Zeit.," says that the latter contains a serio-comic article on this discovery of Lostorfer, which, for a few moments, shook such men as Stricker, Hebra, and Skoda off their

balance. Dr. Wedl, the author of the article, says:—"These corpuscles will, at the next meeting of the Medical Society, be solemnly buried in Dr. Stricker's museum. The sympathy of the profession is requested under these painful circumstances, and the Society will doubtless institute special masses for the repose of the departed. It is very lucky that the members did not, in their hurry, have medals struck for the discovery; and there is time left to send counter-orders to Paris and London, to stop enthusiastic researches which might bring some blame on the Vienna Medical Society. The latter may derive from this mishap the lesson to beware of allowing itself to be made a trumpet to ephemeral discoveries." The "Lancet," however, finds it difficult to believe—and the "Microscopical Journal" quite coincides with it—that observers like Stricker and Hebra would have been carried away by imperfect experiments. It is clearly stated that different kinds of blood were placed under Dr. Lostorfer's microscope (he not knowing whence the blood came), and he constantly recognised his peculiar corpuscles in blood coming from patients affected with syphilis.

Colourless Bile.—In the "Comptes Rendus," March 18, M. E. Ritter quotes the results of a series of analyses made by him on colourless bile, taken from the gall-bladders of men and animals submitted to autopsy. As an instance of the composition of such bile (as yet hardly ever analysed, since the colourless fluid has been taken to be mucus) we mention here the following, in 1,000 parts:—Water, 923·5; salts, 12·4; fat and cholesterine, 6·8; organic matter, 2·1; salts of the bile acids, 55·2. It appears that colourless bile and fatty degeneration of the liver are somehow connected together.

Ancient Egyptian Perfume.—Dr. Personne states in the "Journal de Pharmacie," March, that he accidentally obtained a small piece of a chocolate-brown substance, which originally was apparently a paste, but is now hard. On further examination it was found to consist of a lime-soap, mixed with myrrh, olibanum, benzoin, and probably some essential oil. Dr. Personne states that at the present day there is sold in Egypt as a perfume a substance of similar composition, and locally known as *Bouh Kourre-bare*, which means perfume from the Arabian frontier.

A Mechanical Means of Lowering the Temperature, which is peculiar, is described in a late number of Pfluger's "Archiv," by Herr Manassein. He states that, if rabbits, seated at ease in a box, were swung in a transverse direction to their length, and with a rapidity of twenty-eight to thirty double swings, at a pendent length of 117 ccm., that the temperature taken in the rectum after the swinging was, by 0·3° to 1.2° C., in the mean by 0·66°, lower than before. The depression of temperature continued from a half to two hours, and was most decided after fifteen minutes of swinging; a longer swinging did not increase the effect. The maximum of the depression of temperature occurred, at first, some time (about thirty minutes) after the cessation of the swinging. The last-named circumstance, as well as that the wrapping of the animals in cotton-batting in no wise hindered the effects, and, on the other hand, that a more rapid swinging appeared less effective, prove that the current of air which is produced by the swinging is not the cause. Swinging in the longitudinal diameter made the animal more afraid and more restless; it had, however, the same influence upon the temperature. The effects of the swinging on rabbits were greater

where the eyes were blinded, and less, on the other hand, in animals in which the respiration was moderately disturbed by the tightening of a cord around the neck, and also in animals only slightly narcotized by morphine. In injections of an ichorous fluid, the feverish increase of temperature produced was lessened by the swinging, and indeed, by repeated swinging, brought to a stop.

Influence of Dr. Wright's Morphiæ Compounds on the Animal Body.—Some experiments on this subject were carried out by Dr. Reginald Stocker, Pathologist to St. Mary's Hospital, and are of interest. He says that doses of 1 decigramme of the compound $C_{68}\ H_{81}\ I\ N_4\ O_{10}$, 4HI from codeia, and of the similar compound from morphia, were given to an adult terrier by the mouth without producing any perceptible effect whatever; when the dose was increased to 3 decigrammes, in each case repeated defæcation in the course of a few hours was produced, the stools being more loose than ordinarily and frequently of a dark greenish colour; no other symptom was noticeable, and no appreciable difference in the action of the two compounds was perceptible. Doses of 5 decigrammes of the compound $C_{68}\ H_{82}\ I_2\ N_4\ O_{10}$, 4HI from each of these sources were given to the same dog by the mouth, with the result of producing similar repeated defæcation in the course of two or three hours; the sole difference discernible between these and the former experiments being that the effect was produced somewhat sooner and was of longer continuance in the latter cases, a result probably produced solely by the larger dose. No material differences were observed between the codeia and morphia derivative. The same dog was employed throughout, two or three days being allowed to intervene between each experiment, so that the animal had recovered from the effects of a former dose before the administration of another.—*Proceedings of the Royal Society*, for April, 1872.

Gastric Juice and Pepsin applied to Wounds.—The "New York Medical Journal" states that there have been performed recently a number of experiments with the above fluids, applied as follows: the gastric juice of dogs was pencilled, at short intervals daily, fifteen to twenty times upon the wounded surface, or small pledgets of cotton were applied, and upon them a second larger layer of wadding dipped in a very dilute solution of muriatic acid. Several experiments were made, especially upon chancres, upon soft chancres in particular. After five to eleven days, commencing cicatrization followed as a rule. The remedy is chiefly indicated in soft chancre, in diphtheria, phagedæna, and nosocomial gangrene.

A Curious Memento of Jenner.—A very interesting memento of the discoverer of vaccination has recently been presented to the Royal College of Physicians by Sir John William Fisher. It consists of a cow's horn, beautifully polished, presented to Sir J. W. Fisher, in the year 1813, by Dr. Jenner, and polished by himself. The gift was made in grateful acknowledgement of services rendered to Jenner's sick children by Mr. Fisher, then a medical assistant in Soho. The horn is now mounted in silver, and bears an appropriate inscription, stating the circumstances under which it was presented to the college. Dr. Burrows, the President, in asking the acceptance of the horn, stated that it was probable—though there was no record of the fact—that the horn had been taken from one of Dr. Jenner's favourite cows on which he made his experiments on vaccination.

Atheroma in the Arteries.—Dr. Moxon has been making some recent researches on this subject, which are of importance. He shows the connexion between inflammation of the arterial tunics, atheroma, and aneurism, and he dwells upon, and accounts for, the relatively greater frequency of the latter affection among soldiers. Dr. Moxon holds (1) that what is called atheroma of arteries is sub-inflammation of various degrees, of which the lower degrees end in fatty degeneration of the coats, along with the inflammatory products; and (2) that the determining cause of this change is mechanical strain, a general altered nutrition—such as obtains in gout, syphilis, &c.—being regarded in the light of a predisposing cause.—Vide *Lancet*, June 8.

Chemical Composition of Pus.—The "British Medical Journal" states, in a recent number (May 28, 1872), that Hoppe-Seyler has obtained results which are interesting in reference to the question of the origin of the pus-corpuscles and their identity with the colourless and lymph corpuscles. He introduced fresh crystalline lenses of the ox into the abdominal cavity of dogs, and analysed them after a period varying from one to fourteen days. As was expected, the lenses became infiltrated with lymph-corpuscles. Glycogen was found in greatest abundance at the eighth day, at which period they contained the greatest number of contractile corpuscles. The glycogen is due to these corpuscles. If the lenses were not plunged immediately into boiling water, but allowed to stand for some time, no glycogen was found, but in its place sugar. In the pus of congestion-abscesses, no glycogen occurred. The occurrence of glycogen, therefore, may be taken as a means of distinguishing lymph from pus-corpuscles. When glycogen is found in abscesses, it will be found to coexist with the presence of numerous contractile corpuscles. Lymph-corpuscles, therefore, by their transformation into rigid pus-corpuscles, become deprived of their glycogen.

Animal Starch.—The "British Medical Journal" in one of its May numbers, gives an account of M. Dareste's researches on this point. It says he has observed granules which present the optical characters of starch in the hen's egg, both when newly laid and during the process of incubation. The granules in the new-laid egg give a blue with iodine; while those observed during incubation sometimes give a blue, but often give a red. The granules appear and disappear several times. The formation of the area pellucida is partly due to the disappearance of the third series of granules. The fourth series form the glycogen in the liver. He attributes the disappearance of starch to its conversion into grape-sugar, and its reappearance to the change of grape-sugar back to starch.

Action of Alcohol on the Body.—This subject is yet far from being exhausted. Dr. Subbotin gives in the "Zeitschrift für Biologie" (Band vii., Heft 4), also "Lancet," June 8, 1872, the details of a considerable series of experiments he has recently performed on rabbits. The mode of detection of the alcohol he employed was its acetification by chromic acid, or rather by bichromate of potash and sulphuric acid, the quantity being determined by the subsequent estimation of the distilled acetic acid by means of soda solution. The respiration experiments were conducted in an apparatus lent to him by Voit, and constructed on the plan of the large apparatus of Pettenkofer and Voit. Alcohol of the strength of 29 per

cent. was injected into the stomach through the exposed œsophagus, and this tube at once ligatured. The results at which he arrived were: That, during the first five hours after the introduction of alcohol into the stomach, a considerable amount escapes by the skin, lungs, and kidneys; that at least twice as much escapes by the lungs and skin as by the kidneys; that the amounts he obtained, showing that from 6·79 to 7·4 per cent. were thus eliminated, were, from various considerations, certainly below the quantities really discharged from the system. These conclusions he arrived at in 1870. Quite recently, however, he instituted another series of experiments, the object of which was mainly to determine for how long a time after ingestion alcohol continued to be excreted by the skin and lungs. In one of these experiments he found that 12·6 per cent. of the alcohol was eliminated in eleven hours and a half through all these channels; in another instance 16 per cent. was eliminated in twenty-four hours, either in the unaltered condition or only changed into aldehyde. Subbotin maintains that alcohol cannot be regarded as in any sense a food, since, under its influence, the metamorphosis of tissue diminishes, the temperature falls, the amount of carbonic acid excreted lessens, and a smaller quantity of urea is discharged. Its action, he thinks, is direct upon the nervous system.

METALLURGY, MINERALOGY, AND MINING.

An Improvement in Blowpipe Operations.—MM. Armin, Junge, and Mitzopulos, of Freiburg, have greatly improved the blowpipe by an apparatus of which the following is a description:—A common wide-mouthed bottle is carefully fitted with a caoutchouc cork bored with two holes, into each of which passes a piece of glass tube bent at a right angle. On to one of these tubes is slipped the caoutchouc tube coming from an ordinary caoutchouc bellows, whilst the other is put in communication with the blowpipe nozzle by means of four pieces of caoutchouc tubing joined by three pieces of glass tube, drawn to a fine point at each end. This forms the main peculiarity of the arrangement. When air is forced into the bottle by the blower, in jerks, it finds a difficulty in escaping as fast as it comes in, on account of the six fine openings in the glass tubes that it has to pass through on its way from the bottle to the nozzle, and it thus acquires a certain pressure in the bottle, and flows out towards the nozzle as a regular blast. The bottle may be about 6 inches high by $3\frac{1}{2}$ inches wide, with a neck $1\frac{1}{3}$ inches in diameter; but of course the dimensions are of no great importance. On the whole a somewhat large bottle is better than a small one. The pieces of glass tubing used are 2 inches long by $\frac{1}{3}$ of an inch in diameter. The apparatus will be stronger if instead of a glass bottle a tin cylinder is used, about 4 inches high by 2 inches in diameter, with two tin tubes opening into its top. Small metal cylinders, with a fine hole at each end, may be used instead of the little glass tubes. A blowing apparatus constructed in this manner will deliver a perfectly regular blast, and will be of practical interest to those who are thinking of working in places where it is impossible to repair the ordinary instruments.—See *Chemical News*, June 7th.

The Metallurgy of Lead.—Mr. John Jex Long read the first part of a paper on this subject before the Glasgow Philosophical Society, March 25, 1872, in the course of which he described the operations which he had personally witnessed at the works of the London Lead Mining Company, at Middleton-in-Teesdale, a few miles from Barnard Castle, where the mining operations were commenced about 170 years ago. He explained the geological position of the lead-bearing rocks in Teesdale, describing the direction, extent, and richness of the mines, and their mode of occurrence in flats, pockets, strings, &c. The annual product of the mines referred to is about 2,000 tons of metallic lead, containing about 9 ozs. of silver per ton, which is separated in the metallic form by Pattinson's process. Before the company obtained any pecuniary return from the mines, they had to expend about 30,000*l.* Mr. Long described his exploration of the Coldberry Mine at Middleton, the mode of working it, and the various mechanical operations by which the mineral is prepared for smelting; and he promised, on a subsequent occasion, to devote the second part of his paper to the consideration of the smelting and refining processes and the extraction of the silver. The paper was profusely illustrated by specimens.

Waste of Sulphur in Mining.—Dr. W. H. Tayler writes to the "Chemical News," May 24, upon this subject. He states that whilst minerals of every description are rising in value, it will scarcely be credited, although such is the fact, that in several of the tin mines in Cornwall, at the present time, a large source of what ought to produce wealth is allowed to be wasted. Large quantities of sulphurous fumes are allowed to pass off daily in calcining the tin stuff, instead of manufacturing it into sulphuric acid. He states that he knows of an instance where three tons of sulphur are daily allowed to escape, which if manufactured into sulphuric acid, the present price of which is 3*l.* 10*s.* per ton, would yield a revenue of more than 12,000*l.* a year. While Spain and Portugal and other parts of Europe are ransacked to find sulphur ores to supply the manufacturers of sulphuric acid, in Cornwall all these sources of wealth are allowed to be wasted.

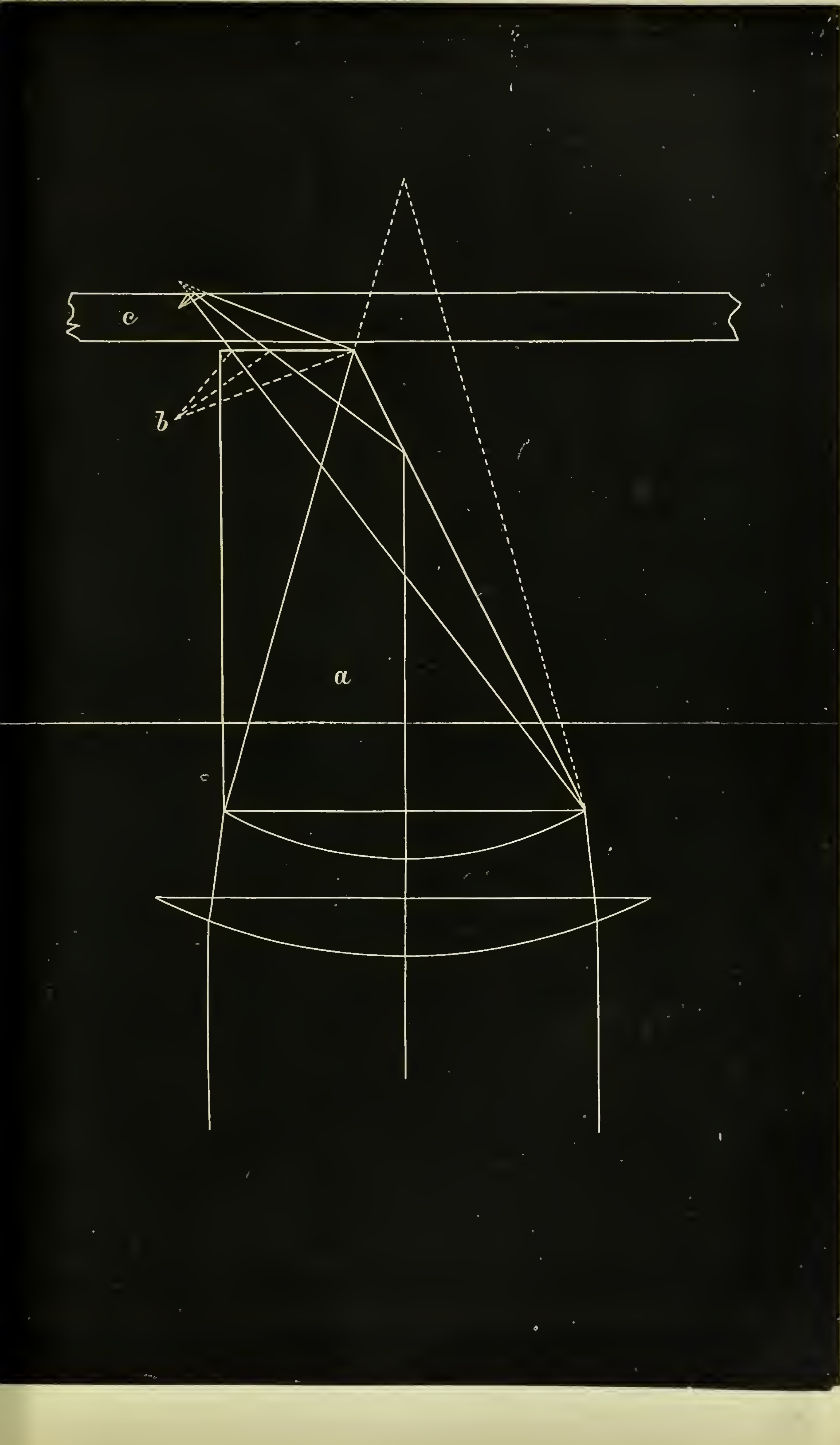
Crystalline Dissociation.—MM. Favre and Valson have published the second part of a very valuable monograph on this subject. It contains so large a series of tabulated forms, the results of experiments, that an abstract would be impossible.

MICROSCOPY.

Wenham's improved Reflex Illuminator for the highest powers of the Microscope.—We cannot resist giving our space to this excellent invention of Mr. Wenham's for microscopic illumination. The diagram (p. 329), five times the size of the instrument, illustrates the plan he has adopted to overcome the defects of the olden apparatus. In this *a* is a cylinder of glass half an inch long and four-tenths in diameter, the lower convex surface of which is polished to a radius of four-tenths. The top is flat and polished. Starting from the bottom edge, the cylinder is worked off to a polished face at an angle of 64°: close beneath the cylinder is set a plano-convex lens of 1¼ focus. Parallel rays sent through the lens, after leaving the lower convex surface of the cylinder, would be refracted to the point shown by the

dotted lines if continued in solid glass; but by impinging on the inclined polished surface (which is far within the angle of total reflection) they are thrown on the flat segmental top; here they would be totally reflected and beaten down again to the point, *b*, outside the cylinder; but if an object-slide, *c*, be laid over the flat top with an intervening film of water, the rays proceed on till the focal point reaches the upper surface or is slightly beyond it; here total reflexion now takes place; all the light is concentrated to a minute spot in the centre of the field of view of the microscope, and most of the rays are available for any object brought there by traversing the slides over the water top of the illuminator, which must be kept full without allowing any to run down the reflecting surface. It will be seen, in order to get the focal point in the centre of the microscope, that the lens centre must be excentric; but this does not involve the slightest inconvenience, as the excentricity only amounts to a little over two-tenths of an inch, and is so small that the same adjustment of the mirror serves during an entire revolution. The apparatus rotates on the focus as a centre. The management of this illuminator is very easy and simple; its fitting goes into the ordinary sub-stage, and has an independent rotary movement of its own, like that of Nachet's prism. The cylinder is brought up nearly level with the stage. The centre of rotation is set true by a dot on the fitting, seen with a 1½ object-glass. A drop of water is then placed on the top, upon which the slide is laid. The required objects *on the slide* are found by a low power, and may be distinguished by their brilliant appearance, while those on the cover are nearly invisible. The light is thrown up by either the plane or concave mirror. The former is generally the best and most controllable. The lamp should not be placed much beyond the stage, else its direct rays will get underneath and mar the blackness of the field. Having got the best effect, say on a *diatom*, or insect scale, by tilting the mirror, we now proceed to rotate the illuminator. During this the most exquisite unfolding of structure takes place, opening out as it were into detail the form of bosses or ribbings. On that superb test, the *Podura*, for example, when the light is thrown from the apex to the quill, the whole scale is dotted over with bright blue spots laying in a zigzag direction; these are the most prominent parts or the club-end of the markings, which are nearest in contact with the glass. Fuller details are contained in the paper from which these remarks are taken, which should be referred to by the reader.—Vide *Monthly Microscopical Journal*, June 1872.

The Best Way to see the Markings on Battledore Scales is the following, according to Dr. Anthony ("Monthly Microscopical Journal," June, 1872). He says that scales are best seen by "transmitted" *ordinary* light, when a "pin-hole" stop is placed like a small cap on the usual wide-angled condenser, and by being very particular that both flame of the lamp and object are in focus, or very nearly so, at the same time. When scales are looked at by "reflected" light, then they are to be seen at their best by bringing up a little transmitted light at the same time, such light being quite subordinate, and only for the purpose of rendering the black shadows transparent. A similar effect of course can be got by using a second lamp and "bull's-eye" at the other side of the microscope, or even in a minor degree by a bit of white card, placed in the stage beneath the object, so as to reflect light, but


c
b
a

on the whole he prefers the first form of "double illumination" as equally satisfactory and far less troublesome.

Mr. Collins' Light Corrector.—An ingenious bit of apparatus has been devised by Mr. Charles Collins (17, Great Portland Street). It consists of a brass stage-plate with a groove in which rotates a diaphragm of four apertures, one of them being open and the other containing blue glasses of special tint, and one with a finely-ground surface. These effectually correct the yellowness of all artificial illumination, making the light soft and agreeable to the eyesight as well as improving the definition. It is in fact an improvement on Rainey's Light Modifier so as to obtain more varied effects, and does not require any special fitting, as it can be used on any microscope.

A Curious Fact in Bichromatic Vision has been observed and described by Mr. J. W. Stephenson, F.R.M.S. He says, as of course is known to most students of optics, that, by the aid of a double-image prism and a film of selenite, two images may be shown in the field of the microscope, the colours of which will be complementary the one to the other, and that when these images overlap, the resulting image will be, as far as the overlapping extends, of white light; but it is not, he thinks, so well known that when, by a suitable arrangement, different colours are made to occupy the two fields of a binocular, the resultant is a combination of such colours, and that if these are complementary to one another, the sensation of colour induced in the brain by the retina of one eye, is neutralised by that which reaches it through the instrumentality of the other, and that by the combination of the two the sensation of colour is entirely lost. This, however, Mr. Stephenson observed in the most convincing way. The fact is a little curious, and we think, as yet, is insufficiently explained.—*Monthly Microscopical Journal*, May 1872.

The Two Best Test Objects.—Dr. Col. Woodward says that *Amphipleura pellucida* is a useful and valuable test for immersion objectives of $\frac{1}{8}$th inch focal length or less. Lower powers can only hope to resolve it if possessed of excessive angular aperture. When, however, it is desired to discriminate between small differences in the excellence of objectives intended for the most exquisite resolution, a more subtle test is required, and this will be found in the nineteen-band plate of Nobert, by those who take proper precautions in its use. Those, however, who believe they have secured resolution whenever they see lines in the higher bands of the plate, without duly considering their number, must not be surprised if objectives they have accepted as resolving the ultimate bands of the plate fail to show the striæ on even the coarsest frustules of the *Amphipleura pellucida.*—*Monthly Microscopical Journal*, April.

PHYSICS.

Death of Professor Morse.—We regret being obliged to announce the death of Professor Samuel Finley Breeze Morse, which took place at his residence in New York city, on April 4, at the advanced age of eighty-one

years. Few Americans have attained so world-wide a renown as Mr. Morse, growing chiefly out of his success in devising and introducing the system of electric telegraphy which bears his name. Mr. Morse was the eldest son of Jedidiah Morse, D.D., an American clergyman, better known as a geographer, whose writings were the first devoted to the elucidation of American geography, his educational works of this character remaining for more than a generation in general use. Prof. Morse was born in Charlestown, Mass., April 27, 1791. He graduated at Yale College in 1810.

An Improvement in Barry's Singing Flame has been devised in America by Mr. W. E. Geyer, and is described by him in "Silliman's American Journal." He states that a simple addition to the apparatus described by Barry last year has given him a flame which, by slight regulation, may be made either: (1) a sensitive flame merely; that is, a flame which is depressed and rendered non-luminous by external noises, but which does not sing; (2) a continuously singing flame, not disturbed by outward noises; (3) a sensitive flame, which only sounds while disturbed; or (4) a flame that sings continuously, except when agitated by external sounds. The last two results, so far as is known to him, are novel. To produce them it is only necessary to cover Barry's flame with a moderately large tube, resting it loosely on the gauze. A luminous flame six to eight inches long is thus obtained, which is very sensitive, especially to high and sharp sounds. If now the gauze and tube be raised, the flame gradually shortens and appears less luminous, until at last it becomes violently agitated, and sings with a loud uniform tone, which may be maintained for any length of time. Under these conditions, external sounds have no effect upon it. The sensitive musical flame is produced by lowering the gauze, until the singing just ceases. It is in this position that the flame is most remarkable. At the slightest sharp sound, it instantly sings, continuing to do so as long as the disturbing cause exists, but stopping at once with it. So quick are the responses, that by rapping the time of a tune, or whistling or playing it, provided the tones are high enough, the flame faithfully sounds at every note. By slightly raising or lowering the jet, the flame can be made less or more sensitive, so that a hiss in any part of the room, the rattling of keys even in the pocket, turning on the water at the hydrant, folding up a piece of paper, or even moving the hand over the table, will excite the sound. On pronouncing the word "sensitive," it sings twice; and in general it will interrupt the speaker at almost every "s" or other hissing sound.

A new Seismograph has been described in a paper before the Wellington Philosophical Society, New Zealand, by Mr. Wm. Skey. The following is a partial account of the instrument. A small block of metal, having a thin slip of platina attached, or a small wire of this metal projecting a little apart from it horizontally, is connected with an electro-magnet with keeper suspended, and this with a single cell of a battery. A very fine silver wire (that used for sewing wounds), 3 ft. long or so, joined at its lower extremity by a little platina wire, depends from a point above, so that the two platina wires may intersect; a firm adjusting screw or other apparatus set contiguous to the point of suspension enables one to put this point in such a position that these wires are allowed to press but very slightly upon each other. The silver wire is connected with the other pole of the cell through

this point of suspension through a vertical galvanometer. The shock-receiving part is placed underground to avoid the interference of winds or that of violent detonations, the metal block being set upon a wooden pile driven some distance in solid earth. When properly set, a single make and break contact of this kind is so sensitive that the impact of 3 pounds of stone, falling from a height of 5 ft. upon the ground, at a distance of 50 ft. from it, moved the needle of the galvanometer very determinately. The intervening ground was clay.

Coal-gas for Lighthouses.—Mr. J. Wigham recently gave a lecture in Dublin on this subject. He said that coal-gas was first used in lighthouses in 1865, by Mr. Samuel Bewley of the Irish Board of Lights, who tried some experiments at Howth. The first burner used was called the "crocus." The principles involved in this burner, and the means taken to economise the gas, proved that by the crocus there was an immense saving, taking light for light, as compared with the gas usually used in our houses. The lecturer then alluded to the economy as compared with oil. There was a saving of about 50*l.* per annum on each lighthouse in which the gas had been tried, and, in the case of intermitting lights, the difference was much greater as regards economy. Dr. Tyndall has been sent down to investigate the whole matter at Howth, and that gentleman had reported favourably. There were five lighthouses at present on the Irish Coast illuminated by gas, and they are about to try it on two of the English lighthouses. In speaking of the electric light, Mr. Wigham said that though the latter was very intense, yet it was deficient in quantity, and it was not so good as coal-gas for penetrating fogs. Mr. Wigham then proceeded to explain the mechanical arrangements and the construction of the lenses.—*Royal Dublin Society,* March 18, 1872.

Estimating the Intensity of Light.—The *Chemical News,* April 12, quoting from an American journal, states that Dr. Vogel proposes *nitroprussid iron* as a suitable salt for determining quantitatively the intensity of light. For the preparation of this reagent, dissolve chemically pure oxide of iron, best obtained from the oxalate, in hydrochloric acid, and evaporate nearly to dryness to expel the excess of acid; and after filtering, add an aqueous solution of nitroprussidnatrium in proportion of three of the iron to two of the latter. There is usually a slight precipitate produced by this mixture, which can be collected on a filter; but this operation must be performed in a dark room. We have now a liquid excessively sensitive to the action of sunlight. By exposing a small quantity of a known specific gravity to the action of light, a precipitate of prussian-blue will instantly begin to fall; and, on redetermining the sp. gr. in the dark chamber, its decrease will be found to be proportional to the precipitate; and we have thus the data for measuring the intensity of light. It was found by Dr. Vogel that the liquid, exposed for forty-eight hours before a kerosene lamp, was not in the least affected, but a piece of magnesium wire, when burned, immediately produced a precipitate. By employing a long instrument graduated in millimetres, it would appear to be possible to measure the intensity of the light by the number of millimetres occupied by the precipitate. The invention has an important bearing upon photography.

How to bend Glass Tubes so as to fit Apparatus.—Mr. J. Laurence Smith

states that it is well known that it requires some tact to bend a tube with an even curve and without collapsing its sides, and many chemists never do succeed in bending them skilfully. Although having no particular skill in this matter, he never fails to bend them perfectly satisfactorily, by using a flame different from the one usually employed; the flame is one given by the Bunsen burner described in his article on alkali determination in silicates (see *Chemical News*, vol. xxiii., p. 235). The burner is very commonly used now in all laboratories, where the extremity of the burner is flattened out so as to give a short and thin but broad flame, something like the flame of an ordinary gas-burner. The tube is placed in this flame and turned round and round, until a good heat is given to the tube; it is then withdrawn from the flame and bent, when it does so with a perfect curve and no collapse of the sides of the tube. Of course this is only intended for the smaller tubes, but a tube of 1 centimetre and more can be thus bent very readily.

Spectra of Manganese in Blowpipe Beads.—Manganese may be easily detected in this manner according to Mr. Charles Horner (*Chemical News*, March 22). The following is the best way of preparing the beads and examining their spectra. Sufficient chlorate of potash should be volatilised in the loop of platinum wire to form a bead about the size of a pin's head, then take up the merest trace of the oxide and fuse it; next add enough chlorate to fill the loop, and very gently flame the bead for a few seconds and withdraw, when it crystallises a delicate pink colour. In adding the second portion of chlorate care must be observed not to volatilise the salt, and the best result is when the bead does not much exceed the thickness of the wire. If after adding the second portion we volatilise the chlorate, we immediately obtain a greenish-coloured bead of manganate of potash, and more transparent than the pink bead. In order to see the spectra of these beads, they should be examined by the spectrum microscope and strongly illuminated. The pink bead exhibits several absorption-bands more or less definite according to the amount of manganese present. The three most distinct bands, however, lie between D and *b*, and may be seen when the bead is scarcely coloured. This spectrum very closely resembles that given by the crystals of perchlorate coloured by permanganate of potash, but the bands are slightly more refrangible in the former. The green bead gives a spectrum of two bands, one broad band covering the sodium line, and a very narrow band in the orange ray. This spectrum test is most useful in the examination of minerals, for although the pink colour is sometimes disguised by the presence of other substances, as in rhodonite, which communicates a yellowish tint to the bead, yet the three principal absorption-bands are plainly visible.

ZOOLOGY AND COMPARATIVE ANATOMY.

Zoological Nomenclature.—This subject has excited considerable attention in the United States. "Silliman's American Journal," May 1872, contains an important review of Mr. Lyman's recent and somewhat novel opinions on the subject. We of course cannot enter on so long a question. We may, however, refer the reader to Mr. Lyman's work, "Illustrated Catalogue of the

Museum of Comparative Zoology," in which, under the heading of "Notes on Nomenclature," his peculiar views are expressed. It is remarkable that some years ago, the American Association for the Advancement of Science appointed a committee to reconsider the canons of biological nomenclature, and to report whether, with the growth of science, they required any additions or alterations. No report has yet been made, nor, so far as we are aware, is any likely to be presented, until the subject is again brought prominently forward and new instructions given. Professor A. E. Verrill has since republished the Revised Rules of Zoological Nomenclature adopted by the British Association for the Advancement of Science in 1865, and has accompanied them by a few apt comments: in England, Mr. W. F. Kirby, in a paper read before the Linnean Society of London, has called attention to the extensive changes which a strict adherence to the laws of priority would cause in the generic nomenclature of butterflies; and quite recently has put the same into practice in his catalogue of these insects.

A New Crustacean: Tomocaris Peircei has been discovered by Professor Agassiz. This, which is named as above, was dredged in 45 fathoms about 40 miles east of Cape Frio. It is described as very like *Serolis*, with the marked difference, that the thoracic rings are much more numerous, and the abdomen much smaller; and it is said that its resemblance to Trilobites is unmistakable and very striking, and that it can be referred to no one of the orders or families in Milne Edwards' or Dana's classification. From the details of Prof. Agassiz's description, the animal is evidently one of the Serolidæ, apparently congeneric, perhaps specifically identical, with the *Brongniartia trilobitoides* of Eights (Trans. Albany Institute, vol. ii., p. 53, pl. 1, 2, 1833), which is referred to the genus *Serolis* by Audouin and Milne Edwards (Archives du Muséum d'Hist. nat., tome ii., p. 29, pl. 2, fig. 11, 1839), and retained in the same genus by Milne Edwards in his great work (Hist. nat. des Crust., tome iii., p. 232, 1840). To make this apparent it is necessary to observe that what Prof. Agassiz calls the head includes the first thoracic segment, which in the *Serolidæ* is anchylosed with the head; that what he considers the three posterior segments of the thorax, have been regarded by carcinologists as belonging to the abdomen; and that, as a result of this first homology, what have been regarded as the anterior legs are called maxillipeds. The only point in the whole description which can militate against the view here expressed is in the description of the nine pairs of legs which are said to be "all alike in structure;" the six anterior pairs, however, are "larger than the three last, which are also more approximated to each other," thus agreeing perfectly in position with the three anterior abdominal legs of the ordinary Serolidæ. The perfect agreement in all other respects, however, leaves little doubt of the close affinity between *Tomocaris* and the *Brongniartia* of Eights. It may be well to notice that among the species referred to *Serolis*, there are several genera, distinct from the typical *S. paradoxa*, and that the species described by Eights represents one of these, although the name *Brongniartia* is preoccupied.—*Silliman's Journal*, May.

The Great Public Aquarium at Naples.—An account of this immense undertaking is given by a contemporary, and is of sufficient interest to have a place in our columns. The building, which is under the

direction of M. Anton Dohrn, is rectangular, measuring 100 ft. by 70 ft., with a height of 40 ft., and is 100 ft. from the sea. The lower part is to be occupied by the tanks of the great aquarium, to be opened to the public; and the upper will contain 24 rooms for laboratories, a library and collections, with lodging rooms for three or four zoologists. There will be 53 tanks in the lower story, one of them 32 ft. long, 10 broad and $3\frac{1}{2}$ deep, another, 26 ft. long, and twenty-six 3 ft. by $3\frac{1}{2}$ ft. The tanks throughout are furnished with a continuous current of sea-water. Upstairs, the library room is large enough to hold 25,000 volumes. The principal laboratory room will contain 20 to 30 tanks of different sizes; and besides there are private laboratories for the chief zoologist and the first assistant, and other small laboratory rooms, and rooms for collections.

Three Additional Zoological Publications have lately made their appearance, and if we may judge from the first numbers and from the character of the editors, they are likely to commend success. The one, "Archives de Zoölogie Expérimentale et Générale," issued by Professor Lacaze Duthiers, will take a high place among scientific periodicals, and is likely, in French zoological literature, to take the position which Siebold and Kölliker's "Zeitschrift" takes in Germany. Professor Lacaze Duthiers, so well known for his thorough researches upon the Invertebrates of the Mediterranean, contributes an introduction to the first number, stating the aims of the publication, and concludes the number by an elaborate article on the auditive capsules of Gasteropoda. The other original article of this number is written by Mr. Perrier, who has given us an excellent paper on the Natural History of a fresh-water worm (Deto) allied to Nais. This periodical, as well as the other "Journal de Zoölogie," published under the auspices of Professor Gervais, both have notes and reviews on scientific works published in countries outside of France, a feature which thus far has received but little attention from French scientific journals. Holland, which already publishes so many scientific memoirs and periodicals of great excellence, adds a purely zoological archive to its list, edited by Professor Selenka. The first number contains a tolerably complete embryology of one of the naked Gasteropods by Selenka, and a long paper by C. K. Hoffman on the anatomy of Echinoderms; both these papers are excellently illustrated. Professor Selenka intends to issue his Niederländische Archiv für Zoölogie whenever sufficient material is at hand; he solicits articles either in German, French, or English.

Appearance of Colour in Fish kept in Alcohol.—Mr. Richard Bliss, writing in the "American Naturalist," April, states that a short time since while examining a number of alcoholic specimens of Cyprinoids from Ogden, Utah, collected by Mr. J. A. Allen last September for the Museum of Comparative Zoology, he noticed a species of *Richardsonius* distinguished by a bright vermilion spot on the abdomen. The size of the spot varied in different individuals: in some it was quite small, in others it extended from the base of the pectoral fin to the anal opening. Calling Mr. Allen's attention to this fact he stated, greatly to his surprise, that this colour was not present in the living fish when he caught them, but appeared after the fish had been in alcohol a short time. A dissection of one of these fishes showed Mr. Bliss that the colour was deposited in the areolar layer or derm, and was

therefore a true pigmentary colour. The only explanation he can offer to account for this peculiar appearance of colour is this:—It is well known that during the breeding season fishes frequently take on the most brilliant colours, which disappear when that season is past. It is not therefore improbable that this colour may have been one, at least, of the colours assumed by the fish during the reproductive period, and that the alcohol served in some way to bring out the colour thus abnormally. Whatever may have been the cause, the fact that colour can so appear in fishes will serve as a caution to ichthyologists when describing species from alcoholic specimens alone, lest they confound abnormal or seasonal colours with those that are permanent.

Parthenogenesis among Lepidoptera.—It seems that the Dutch naturalist, M. H. Weizenbergh, jun., has performed a series of experiments on this interesting subject, the insect placed under observation being *Liparis dispar*, and concludes that it is possible for at least three successive generations to be produced without access of the male to the female. The following are the results of his very careful experiments:—(1) August 1866, eggs laid by impregnated females; April 1867, caterpillars appear, and in July perfect butterflies. (2) August 1867, eggs laid by females of this year are without impregnation; April 1868, caterpillars appear, and in July perfect butterflies. (3) August 1868, eggs laid by females of this year without impregnation; April 1869, caterpillars appear, and in July perfect butterflies. (4) August 1869, eggs laid by the females of this year without impregnation; April 1870, no results, the eggs all dried up. The power of reproduction appeared to decrease year by year when impregnation was prevented. Similar results have been noticed in other butterflies, in bees, and notably in aphides.

An Error of Mr. Darwin's.—The "American Naturalist" (May 1872) states, that in the last edition of his "Origin of Species," Mr. Darwin misstates Hyatt and Cope's law of acceleration and retardation in the following language: "There is another possible mode of transition, namely, through the acceleration and retardation of the period of reproduction. This view has lately been insisted on by Prof. Cope and others in the United States. It is now known that some animals are capable of reproduction at a very early age, before they have acquired their perfect characters," &c. A writer, who signs himself "Z," states, that Prof. Cope and others have not insisted on the above propositions, which he imagines to be supported by very few facts. Their theory of acceleration and retardation states, that, while the period of reproductive maturity arrives at nearly the same age or period of the year in most individuals of a single sex and species, the portion of the developmental scale which they traverse in that time may vary much. That an addition to the series of changes traversed by the parent would require, in another generation, a more rapid growth in respect to the series in question, which is *acceleration.* A falling short of accomplishing that completeness would result from a slower growth, hence the *process* is termed retardation. Vast numbers of observed facts prove that this is the great *law of variation*, towards which little progress has yet been made by students who are yet chiefly occupied with the co-operative law of natural selection.